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JABEZ NORTH JACKSON

PRESIDENT 1913

TWENTY-THIRD ANNUAL MEETING

TRANSACTIONS OF THE

WESTERN SURGICAL ASSOCIATION

Twenty-Third Annual Meeting

St. Louis, Missouri, December 19 and 20, 1913

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OFFICERS AND EXECUTIVE COUNCIL, 1914

PRESIDENT

Byron B. Davis, M. D. - - - Omaha

FIRST VICE-PRESIDENT

Leonard Freeman, M. D. - - - Denver

SECOND VICE-PRESIDENT

Lawrence W. Littig, M. D. - - Davenport

SECRETARY-TREASURER

Arthur T. Mann, M. D. - - Minneapolis

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Jabez N. Jackson, M. D. (Chairman) Kansas City

Lewis L. McArthur, M. D.

Amos W. Abbott, M. D. - - Minneapolis

William W. Grant, M. D. - - - Denver

Malcolm L. Harris, M. D. - - - Chicago

CHAIRMAN COMMITTEE OF ARRANGEMENTS

William W. Grant, M. D. - - - Denver

Dr. Ford 6/23/23

EX-PRESIDENTS AND MEETING PLACES

PRESIDENT	PLACE	YEAR
S. S. Todd*	Topeka	1891
Milo B. Ward*	Kansas City	1892
Milo B. Ward*	Des Moines	1893
Lewis Schooler	Omaha	1894
John E. Summers, Jr.	Kansas City	1895
Thomas J. Beattie	Topeka	1896
Joseph Eastman*	Denver	1897
David S. Fairchild	Omaha	1898
Homer C. Crowell	Des Moines	1899
O. Beverly Campbell	Minneapolis	1900
August F. Jonas	Chicago	1901
James E. Moore	St. Joseph	1902
Alex. Hugh Ferguson	Denver	1903
Charles H. Mayo	Milwaukee	1904
Harvey D. Niles*	Kansas City	1905
Malcolm L. Harris	Salt Lake City	1906
Charles W. Oviatt*	St. Louis	1907
William W. Grant	Minneapolis	1908
Arthur L. Wright*	Omaha	1909
John P. Lord	Chicago	1910
Amos W. Abbott	Kansas City	1911
Louis W. McArthur	Cincinnati	1912
Jabez N. Jackson	St. Louis	1913

*Deceased.

The Western Surgical Association does not hold itself responsible for, or necessarily endorse, any of the papers printed herein.

ARTHUR T. MANN, Secretary-Treasurer.

CONTENTS

Officers and Executive Council for 1914 5
Ex-Presidents and Meeting Places 7
Transactions 9

PAPERS

President's Address. Jabez N. Jackson, M. D., Kansas City, Mo. 21

The Treatment of Blood-Vessel Injuries. W. W. Grant, M. D., Denver, Colorado 29

The Use of the Wire Tourniquet in Operations for Goiter. Leonard Freeman, M. D., Denver, Colorado 39

Treatment of Tuberculous Hip-Joint Disease with Coexisting Sinus by Means of Bismuth Paste, with a Report of One Hundred Cases. Emil G. Beck, M. D., Chicago, Illinois 45

Report of Interesting Bone Cases. By William Hessert, M. D., F. A. C. S., Chicago, Illinois 67

Parosteal Callus: The So-Called Myositis Ossificans Traumatica. (A preliminary report.) Oliver J. Fay, M. D., Des Moines, Iowa 85

Studies on the Function of the Periosteum. James E. Moore, M. D., Chief of the Department of Surgery, University of Minnesota, and J. Frank Corbett, M. D., in Charge of the Experimental Division, University of Minnesota 97

Subacromial Bursitis. L. W. Littig, A. M., M. D., M. R. C. S., Davenport, Iowa 107

Multiple Exostoses: An Hereditary Affection of the Bony Skeleton. By Roger T. Vaughan, M. D., Chicago, Illinois 115

Avulsion of the Scalp. Arthur Ayer Law, M. D., F. A. C. S., Minneapolis, Minnesota 125

Gas-Bacillus Infections, with Report of Cases. W. D. Haines, M. D., Cincinnati, Ohio 135

The Significance of the Jackson Veil. Daniel N. Eisendrath, M. D., and E. W. Schnoor, M. D., Chicago, Illinois 142

Pericolic Membranes. Joseph Rilus Eastman, M. D., Indianapolis, Indiana 152

The Significance of Intra-Abdominal "Bands," "Folds," and "Veils." William Seaman Bainbridge, A. M., Sc. D., M. D., New York City, N. Y. (Presented by Invitation) 161

The Best Method of Suspending the Right Colon. Herman E. Pearse, M. D., Kansas City, Missouri..... 199

Cholecystitis and the Factors that Control Results of Operation. Charles H. Mayo, M. D., Rochester, Minnesota 207

Repair of the Common Duct by Means of Transplanted Fascia. Carl B. Davis, M. D., and Dean D. Lewis, M. D., Chicago, Illinois..... 217

The Duodenum; Mobilization, Traumatic Rupture, and Toxemia. A Consideration of Mobilization of the Third Portion of the Duodenum, with Remarks on Rupture of this Part and Duodenal Toxemia. Allen B. Kanavel, M. D., Chicago, Illinois..... 221

Pseudopancreatic Cysts, with a Report of Four Cases. Frederic A. Besley, M. D., Chicago, Illinois..... 235

Congenital Pyloric Stenosis. Roland Hill, M. D., C. M., St. Louis, Missouri 243

Ulcer of the Stomach in Children Before Puberty. Charles D. Lockwood, M. D., Pasadena, California... 255

Retrocecal Appendicitis. David Walker Basham, M. D., Wichita, Kansas 277

The Removal of the Appendix in All Cases of Appendicitis with Localized Abscess. Van Buren Knott, M. D., Sioux City, Iowa 283

Surgical Handling of the Pus-Infected Peritoneum. Thomas C. Witherspoon, M. D., Butte, Montana..... 301

Best Methods of Discouraging the Activity of Inoperable Cancer. A Study of Heat in Cancer. James F. Percy, M. D., Galesburg, Illinois..... 311

Sarcoma of the Ovary. Miles F. Porter, A. M., M. D., Ft. Wayne, Indiana 329

Non-Papillary Benign Tumors of the Bladder. E. S. Judd, M. D., Rochester, Minnesota 343

The Necessity of Conserving the Intercostal Nerves in Abdominal Incisions: An Experimental Study. E. P. Quain, M. D., Bismarck, North Dakota..... 353

Coincidence of Umbilical Hernia with Gall-Stones. John C. Hancock, A. B., M. B., F. A. C. S., Dubuque, Iowa 373

Conservative Operative Treatment of Long-Standing Inversion of the Uterus. H. S. Crossen, M. D., St. Louis 379

Suprapubic Cesarean Section for Puerperal Eclampsia. B. M. Ricketts, M. D., Cincinnati, Ohio..... 389

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Executive Session

December 19, 1913—First Day—Morning Session

The Association met at the Planters Hotel, St. Louis, Missouri, at 9:00 A. M., and was called to order by the President, Dr. Jabez N. Jackson, of Kansas City, Missouri.

Dr. W. W. Grant, of Denver, read a paper entitled "The Treatment of Blood-Vessel Injuries," which was discussed by Dr. J. Frank Corbett, Minneapolis.

Dr. Leonard Freeman, of Denver, read a paper entitled "The Use of the Wire Clamp in Operations for Goiter."

Dr. Emil C. Beck, of Chicago, asked that his paper be taken up later, owing to the non-arrival of patients whom he wished to exhibit before the Association.

Dr. William Hessert, of Chicago, read a paper entitled "Some Cases of Bone-Transplantation and Fractures."

This paper was discussed by Drs. A. E. Hertzler, A. F. Jonas, George C. Gray, C. E. Black, Leonard Freeman, James E. Moore, W. T. Reynolds, Willard Bartlett, Clarence Nicholson, M. L. Harris, D. S. Fairchild, and the discussion closed by the author of the paper.

Dr. Emil G. Beck, of Chicago, read a paper entitled "Treatment of Sinuses from Hip-Joint Disease by Means of Bismuth Paste, with Report of One Hundred Cases."

In connection with the reading of his paper Dr. Beck presented patients.

The paper was discussed by Drs. J. W. Cokenower, W. T. Reynolds, and, in closing, by the essayist.

At this juncture Dr. J. Rilus Eastman, First Vice-President of the Association, took the Chair and stated that it had been suggested that the papers of Drs. Fay and Moore and Corbett be discussed together with the paper of Dr. Vaughan.

This was taken by consent.

Dr. J. Frank Corbett then read a paper entitled "Studies on the Function of the Periosteum," this being a joint paper with Dr. James E. Moore, of Minneapolis.

This was followed by the paper of Dr. Oliver J. Fay, of Des Moines, Iowa, entitled "Traumatic Parosteal Bone and Callus Formation."

Dr. Lawrence W. Littig, of Davenport, Iowa, read a paper entitled "Subacromial Bursitis."

Dr. Roger T. Vaughan, of Chicago, followed with a paper entitled "Multiple Exostoses."

These papers were discussed by Drs. A. A. Law, A. E. Hertzler, Emil G. Beck, M. L. Harris, D. S. Fairchild, James F. Percy, Oliver J. Fay, J. Frank Corbett, and, in closing, by Dr. Roger T. Vaughan.

Dr. A. A. Law, Minneapolis, Minnesota, read a paper entitled "Avulsion of the Scalp."

This paper was discussed by Drs. George C. Gray, Leonard Freeman, and, in closing, by the essayist.

On motion, the Association then adjourned until 2:00 P. M.

First Day—Afternoon Session

The Association reassembled at 2:00 P. M., and was called to order by the Second Vice-President, Dr. Major G. Seelig, of St. Louis.

Dr. Willard D. Haines, of Cincinnati, Ohio, read a paper entitled "Gas-Bacillus Infection, with a Report of Cases."

This paper was discussed by Drs. Byron B. Davis, C. E. Black, and, in closing, by the author of the paper.

The President, Dr. Jabez N. Jackson, Kansas City, Missouri, delivered his address, after which he resumed the Chair.

The next order was a "Symposium on Jackson's Veil or Pericolic Membrane," opened by the members of the Committee, Dr. Daniel N. Eisendrath and Dr. J. Rilus Eastman, followed by a general discussion in which the following gentlemen took part: Drs. L. A. Greensfelder, William Seaman Bainbridge, Leonard Freeman, F. Gregory Connell, Miles F. Porter, M. L. Harris, Van Buren Knott, Clarence M. Nicholson, Thomas C. Witherspoon, Arthur N. Collins, Byron B. Davis, A. A. Law, William T. Reynolds, A. A. Kerr, Clifford U. Collins, and Jabez N. Jackson.

Dr. Eisendrath suggested that the investigation be continued, and that the report be made at the next annual meeting.

Further discussion was participated in by Drs. Joseph Rilus Eastman, M. L. Harris, and William Seaman Bainbridge.

Dr. Herman E. Pearse, of Kansas City, Missouri, read a paper entitled "The Best Method of Suspending the Colon," which was discussed by Dr. Donald Mac-

rae, Council Bluffs, Iowa, and in closing, by the author of the paper.

On motion, the Association adjourned until 9:00 A. M., Saturday.

December 20—Second Day—Morning Session

The Association met at 9:00 A. M., and was called to order by the President.

Dr. Charles H. Mayo, of Rochester, Minnesota, read a paper entitled "Observations on Cholecystitis and the Factors Controlling Results of Operation."

This paper was discussed by Drs. Daniel N. Eisendrath, Byron B. Davis, and, in closing, by Dr. Mayo.

Dr. Carl B. Davis and Dean D. Lewis, of Chicago, read a joint paper entitled "Repair of the Common Duct by Means of Transplanted Fascia," which was discussed by Arthur T. Mann, E. S. Judd, and, in closing, by the author of the paper.

Dr. Allen B. Kanavel, of Chicago, read a paper on "Exposure and Mobilization of the Third Portion of the Duodenum and Duodenal Toxemia."

The paper was discussed by Drs. Warren A. Dennis, Charles H. Mayo, R. C. Coffey, W. W. Grant, and, in closing, by the essayist.

Dr. Frederick A. Besley, of Chicago, read a paper entitled "A Discussion of Pseudo-Pancreatic Cysts, with a Report of Four Cases."

This paper was discussed by Dr. D. W. Brockman and, in closing, by the author of the paper.

Dr. Roland Hill, of St. Louis, Missouri, read a paper entitled "Congenital Pyloric Stenosis."

Dr. Charles D. Lockwood, of Pasadena, California, read a paper entitled "Ulcer of the Stomach in Children Before Puberty."

These two papers were discussed together by Drs. H. M. Richter, Daniel N. Eisendrath, Charles H. Mayo, Willard Bartlett, Willard D. Haines, E. P. Quain, Clarence M. Nicholson, and, in closing, by Drs. Hill and Lockwood.

Dr. Edward S. Judd, of Rochester, Minnesota, read a paper entitled "Benign Non-Papillary Tumors of the Bladder," which was discussed by Drs. William Jepson and A. C. Stokes.

Dr. D. W. Basham, of Wichita, Kansas, read a paper on "Retrocecal Appendicitis."

Dr. Van Buren Knott, of Sioux City, Iowa, read a paper entitled "The Removal of the Appendix in All Cases of Appendicitis with Localized Abscess."

These two papers were discussed together by Drs. J. Rilus Eastman, Clifford U. Collins, Miles F. Porter, Leonard Freeman, W. W. Grant, Charles H. Wallace, William T. Reynolds, George M. Gray, Charles E. Bowers, James E. Moore, and, in closing, by Dr. Knott.

The Secretary presented the report of the Council as follows:

Report of the Executive Council

There were present at the meetings of the Executive Council Drs. Abbott, Jackson, Grant, Harris, Percy, Fairchild, and Mann.

Sir Arbuthnot Lane, through Dr. William Seaman Bainbridge, sent greetings to all members of the Association present, and a suitable reply was sent to Sir Arbuthnot.

The following are recommended for membership:

Theodore F. Riggs.....Pierre, S. D.
John L. Evans.....Wichita, Kas.
Edward Evans .....La Crosse, Wis.
Alex R. Colvin.....St. Paul, Minn.
John C. Rockafellow.....Des Moines, Iowa
Emil H. Borkman.....Rochester, Minn.
Frank G. Nifong.....Columbus, Mo.
Clarence F. Toland.....Los Angeles, Calif.
Max W. Myer.....St. Louis, Mo.
John C. Oliver.....Cincinnati, O.
Joseph Ransohoff.....Cincinnati, O.
Lewis J. Ransohoff.....Cincinnati, O.
Charles A. L. Reed.....Cincinnati, O.
Neil John MacLean...Winnipeg, Canada (Honorary member)

J. Clark Stewart,* transferred from active to honorary membership.

The following are recommended as officers for the coming year:

President—Byron B. Davis, Omaha.
First Vice-President—Leonard Freeman, Denver.
Second Vice-President—Lawrence W. Littig, Davenport.
Secretary and Treasurer—Arthur T. Mann, Minneapolis.
For the Executive Council W. W. Grant and M. L. Harris as the two members to be elected.

The treasurer's report was approved and is as follows:

The Treasurer's Report

RECEIPTS

Balance on hand Dec. 19, 1912	$541.37	
Fees and dues to Dec. 20, 1913	1,339.90	
Guest tickets to Banquet	40.00	
		$1,921.27

DISBURSEMENTS

Paid for printing	$62.75	
Paid Wm. Whitford for reporting Cincinnati meeting	166.18	
250 vols. Transaction and cuts	762.90	
Honorarium to Secretary	100.00	
Expenses connected with Committee on Jackson's Membrane	59.07	
Buttons	25.00	
Postage, express, etc.	27.80	
Refund on banquet ticket	5.00	
Miscellaneous at Cincinnati	10.00	
		$1,633.70
		$287.57

In accordance with the resolutions passed last year in regard to the practice of fee-splitting, these resolutions were sent to the president or the secretary of all the state university medical schools, to other important medical schools, and to the large surgical societies. They created a great deal of interest, and in many cases led to an active consideration of the question.

It is recommended that the Committee on Cancer be discharged.

As a place of meeting for 1914 the committee recommended Denver with W. W. Grant as chairman of the Committee of Arrangements.

On motion of Dr. Charles H. Mayo, the report of the Council was adopted.

Dr. C. H. Wallace: One of the former Presidents of the Association, Dr. O. Beverly Campbell, is very

ill, and I move that the Association through the Secretary express its sympathy to Dr. Campbell with best wishes for a speedy recovery.

Motion seconded and carried, and the Secretary was so instructed.

On motion the Association adjourned until 2:00 P. M.

Second Day—Afternoon Session

The Association reassembled at 2:00 P. M., and was called to order by the President.

Dr. Leonard Freeman, of Denver, Colorado, announced that the National Clinical Congress would hold its meeting next April in New York, and that the members would be the guests of the American Surgical Association, which met at the same time. The American Surgical Association would be glad to cooperate with them in every way it could.

A trip abroad, as suggested in President Jackson's address, was discussed by Drs. Charles H. Mayo and Willard Bartlett, who had been abroad with the Clinical Congress.

About twenty members signified their desire to go this summer if such a trip could be arranged. Accordingly, a motion was made that a committee be appointed by the incoming President, Dr. Davis, to formulate details of a prospective trip abroad for clinical study.

Dr. Davis was then inducted into office as President, and after thanking the Association for the great honor conferred upon him, the scientific program was proceeded with.

Dr. Walter B. Dorsett, of St. Louis, Missouri, read the report of the Committee on Cancer of the Uterus, as follows:

Report of the Committee on Cancer of the Uterus

Dr. Walter B. Dorsett,
Linmar Building,
St. Louis, Mo.

Dear Dr. Dorsett: In accordance with our conversation held in Chicago in reference to a report of the Committee on Cancer to be made before the next meeting of the Western Surgical, I would say that in my opinion the best the Society can do is to indorse the work on cancer being done by the American Society for the Control of Cancer. I understand that all the national surgical societies are going to take this course. This society has paid officers, and are entering upon the work with great vigor, and I believe getting results. I would suggest that you have the Western Surgical Association pass the following:

Resolved: That the Western Surgical Association now assembled has watched with great interest and satisfaction the formation of the American Society for the Control of Cancer. We are familiar with the objects with which it was formed and believe that there is urgent need for its existence. We know those who are charged with the management of the Society, and believe that they will be honest in their efforts and steadfast in their purpose to check, and it is hoped eventually to control the ravages of that scourge which constitutes the common enemy of all mankind. We pledge our loyal support to the American Society for the Control of Cancer and earnestly commend their efforts to thinking people of all professions everywhere. We further request that Drs..... represent the interest of our Association in the Executive Committee of the American Society for the Control of Cancer for the coming year.

I would suggest that two or more members of the Western Surgical Association be appointed from the list which I enclose. I have left blanks so that you can put in the names.

Trusting that this arrangement will be satisfactory, and with apologies that I cannot be present at the meeting in St. Louis, I remain,

Yours very truly,

Reuben Peterson.

At the conclusion of the report Dr. Dorsett moved the adoption of the resolution with the amendment that the President appoint this committee.

Motion seconded and carried.

The President appointed on this committee Drs. Reuben Peterson, A. W. Abbott, and Walter B. Dorsett; and on motion the appointments were ratified.

Dr. James F. Percy, of Galesburg, Illinois, read a paper entitled "Inoperable Cancer: The Best Methods of Discouraging Its Activity—a Study of Heat and Cancer."

The paper was discussed by Drs. A. W. Abbott, Emil G. Beck, William S. Bainbridge, and, in closing, by the author of the paper.

Dr. Miles F. Porter, of Ft. Wayne, Indiana, read a paper entitled "Sarcoma of the Ovary," which was discussed by Dr. Willard D. Haines and, in closing, by the essayist.

Dr. Eric P. Quain, of Bismarck, North Dakota, read a paper entitled "The Necessity of Preserving the Intercostal Nerves in Abdominal Incisions."

The paper was discussed by Clifford U. Collins, J. Frank Corbett, Daniel N. Eisendrath, and, in closing, by the essayist.

Dr. John C. Hancock, of Dubuque, Iowa, read a paper entitled "Coincidence of Umbilical Hernia with Gall Stones."

Dr. H. S. Crossen, of St. Louis, Missouri, read a paper entitled "The Treatment of Long-standing Inversion of the Uterus," which was discussed by Drs. Walter B. Dorsett, Warren A. Dennis, D. W. Brockman, and, in closing, by the essayist.

Dr. B. Merrill Ricketts, of Cincinnati, Ohio, read a paper on "Surgery of Puerperal Eclampsia; Suprapubic Cesarean Section."

This paper was discussed by Drs. James F. Percy, Miles F. Porter, and, in closing, by the essayist.

Dr. W. W. Grant offered the following resolution of thanks:

RESOLVED: That we thank the profession of St. Louis for their magnificent courtesy, and for the gracious manner and very generous hospitality which they have extended to this Association.

I feel I voice the sentiment of every one in saying that we always feel at home in St. Louis, and that we are always treated so kindly and so well that we go away each time wishing to return at no distant date.

The resolution was seconded by several members, and was carried unanimously.

Dr. B. Merrill Ricketts moved that a vote of thanks be extended to Dr. Bainbridge for coming to St. Louis, and enlightening the Association upon the subjects presented by him.

Motion seconded and carried.

Dr. John P. Lord: Since our last meeting we have lost one of our ex-presidents. I move that the Secretary formulate a suitable resolution on the death of Dr. A. L. Wright, of Carroll, Iowa, and forward a copy of the same to his widow, and that this resolution be engrossed in our records.

Motion seconded and carried.

In the death of Dr. Arthur L. Wright, The Western Surgical Association has lost one of its oldest and most beloved members. He was a man of sincere purpose, modest in his bearing, and upright in all his dealings with his fellows. He

was active in the interests of the Association, and became its president in 1909.

Dr. Wright was conspicuausly faithful and earnest in the fulfillment of the smaller, as well as of the greater, obligations which a long life of active service was constantly placing on his willing shoulders.

We wish to express our deep sympathy with the members of his bereaved family, our earnest appreciation of the many admirable qualities of the doctor as a man and a physician, and our great sense of loss in his removal from our midst.

To this end we are sending a copy of these resolutions to the family and also will incorporate them in the minutes of the Association, and publish them in the next volume of our transactions.

On motion, the Association then adjourned to meet in Denver, Colorado, December, 1914.

President's Address

Jabez N. Jackson, M. D.

KANSAS CITY, MO.

Perhaps the most important development of the past year affecting the profession of surgery has been the practical crystallization of effort to correct some of the abuses which have developed in its practice and which threaten to rob it of its high character and repute. So rapid have been the strides of progress in the science of surgery and its application that the time and thought of the profession have been entirely engrossed therein. Within the brief space of a scant generation, surgery has undergone so wonderful a revolution attended by so magnificent a progress as to make its achievements rank foremost among the marvels of a marvelous age. The discovery of anesthesia less than a century ago marked in itself a memorable epoch in the history of surgery. Through it the horrors of anguishing pain were banished, and with it much of its attendant shock. The victim of disease or injury requiring the ministrations of surgery no longer shrank in terror from the tortures which threatened, but, in peaceful sleep, he yielded his body to the ministrations required.

Notwithstanding this merciful revolution in surgery, however, its sphere of possibilities was but slightly affected. The dread specter of impending infection and death still stood to stay the hand of effort and to chill the spirit of ambition. The surgeon of that day

must indeed be one of hardy soul and nerve of steel; and few there were who were brave enough to dare.

With the immortal discoveries of Pasteur, and the prompt application by Lister of their principles in the prevention of infection, arose the sun of a new day, whose splendors were destined to revolutionize surgery and to challenge the admiration of the world. In a moment, as it were, the shackles were stricken off, and a boundless range of possibilities opened up; diseases hitherto hopeless lifted their cry to surgery for succor; and fields forbidden welcomed its advent with promise. Knowledge opened its springs of invitation with life as its reward. Pathology called to him for aid in unmasking the hidden places whence Death makes first assaults; and Physiology beckoned to the wondrous mysteries of Life.

Surely, a wondrous opportunity; surely, an absorbing task. How promptly surgery has met this opportunity, and how faithfully it has performed its task, are matters of countless record and proclaimed by every tongue. It does not need for us to boast. To the bright pages of these records American surgery has surely contributed its part; and on these pages, we are proud to say, are emblazoned high the names of numerous Fellows of the Western Surgical Society.

But it is not our purpose here to eulogize to you the achievements of surgery. What has been done is well known to all of you, many of you being the doers. Nor is it needed now to eulogize any. So absorbing has been the interest in the work, even to the lay mind, that the world at large knows, almost as well as we, the record of accomplishment, and seeks in general custom with eager anticipation the benefits which surgery bestows.

But what of the hour? Is it not meet that once in a while we halt in the campaign of our progress and

inspect the character of our forces? To every victorious army there become attached in time camp-followers, who, beneath the protection of its glorious flag, find cover for pillage and perfidy, and whose crimes besmirch the record so hardly won. These must be found and driven out lest the moral tone of the corps be wrecked. And so it is today in surgery: The comparative safety of its employment, the sleeping innocence of the victim, the wide field of its opportunities, the dazzle of its achievement, and the faith of its clients have attracted to its ranks a host of incompetents and a preying crew of crooks. In the strife of battle and in the quick advance we have had but little time to give to these. But the rapid, changing period of revolution is largely past. The peaceful time of evolution is at hand. Inspection is in order.

While great have been our accomplishments, and low the mortality of modern surgery, it is not yet, and never will be, a fit field for trifling. In the hands of masters it must yet be recognized that life is at stake in every operation, and that the best is none too good.

Mere cleanliness in surgery may permit an inexperienced and untrained operator to do surgery, and yet not have die more than one in four, or, rather better, one in ten. And yet this means from ten to twenty-five deaths in every one hundred. On the other hand, the skilled surgeon of training and judgment will lose not more than from one to five in every hundred. Who can estimate the value and importance of the ten to twenty whose lives are thus needlessly lost? Yet, oftener than life, both health and happiness are involved, and the assurance of these ultimate ends requires ofttimes more skill than the mere safe ending of the immediate operation. How many there are whose lives of comfort and health are sacrificed to the ambition of an untrained tyro! Nor should

surgery be undertaken even by the best except upon well-matured judgment, supplemented by the aid of all the accessory means of diagnosis, often requiring the collaboration of skilled special investigators. Yet how many offer up their bodies daily to the exploration of men not even trained in the methods of ordinary simple diagnosis! Dimmed by the general glamour of surgery the masses of people have not, in the past, stopped to weigh these considerations.

But, surely as time is, is the fact that the day of awakening *has begun,* and the people are looking to the aid of the expected character of our profession for protection. There was a day when clinical training was not available, and each must largely train himself, *at the expense* of his patient; it is sadly true, but there was no other way, and one could claim little advantage over the other so far as primary training was concerned. But that day has gone. The multiplication of large hospitals with great wealth of clinical material in the hands of skilled operators,—surgeons and teachers,—and with endowed laboratories and abundance of pathological material, makes it possible for all who would or should engage in surgery to secure ample opportunity for training. Nor should a mere six weeks' course in a so-called post-graduate school and hospital be offered as an evidence of training. The rather elaborate and ornate diplomas from such schools of post-graduate instruction where no absolute supervision or instruction is required, has probably done more to deceive the people concerning the presumed surgical ability of the incompetent than any other one agency. We believe in the post-graduate school and in its opportunities. In so far as its courses go, they benefit all and are unquestioned blessings; but we believe the time has come when, except upon the qualifications of a real specified, enforced, supervised, and examined course, the issuance of a diploma should

cease. The incompetent should not be branded with a presumed brand of competency. On the other hand, we believe that the time has also come when the man who has honestly trained himself to be a competent and trustworthy surgeon *is entitled* to some distinction which will speak such training. And, believing this, we feel that the inauguration this year of the "American College of Surgeons" with the authority to confer the degree of Fellow is a most happy event in the history of American surgery. As such, it should have our hearty support until it becomes established upon a secure foothold of permanency, and its tenets are thoroughly understood and appreciated by the people of our country, as well as by the entire profession.

But it is not only incompetency which assails the good reputation of surgery. *Dishonesty* is equally, nay, more dangerous. The general medical profession rests for its security absolutely upon the faith of our clients,—faith in our ability, faith in our honesty, and faith in our devotion solely to their interests. Professional capacity cannot be susceptible of proof to a lay intelligence except in a minor degree. Few actually see the table-side work of the surgeon. Fewer are capable of judging of it intelligently when they do see it. They must *trust*—trust us, trust him by whom they are referred to us, trust our mutual devotion to their interest as the first and sacred charge. The responsibility is inestimable, and, when it is overlooked, set aside, and bartered away, the crime of betrayal of a trust is scarcely less heinous than that of the betrayal of life itself. What, therefore, can anyone say in defense of the modern child of commercial dishonesty, the "commission business," or the custom of secret "fee-splitting," which has become altogether too prevalent even among presumably good surgeons in our country? Is it excusable as "a conventional

crime"? This plea did not save a number of the distinguished citizens of Missouri from the penitentiary. Is it excusable because some one else does it, or because it has become customary in some communities? Is it excusable because the referring agent is entitled to a certain amount? If so, why should he not charge and be paid—honestly? Is it honest that, since you owe me ten dollars and will not pay it, I should go down into your pocket while you are asleep and take it?

This is a practice which leads to several indictments:

First—It induces family physicians to betray the sacred confidences of their trusting patients and refer them to men, not the most competent, not who offer the greatest probability of the saving of life or the preservation of health, but to men who pay for the victim. Life *risked* for money! Is it much worse to *take* life for money?

Second—It lowers the dignity and value of the services of the family physician, who is *presumably* rendering valued services for nothing, and gives a magnified value to surgery, which is presumably receiving all. How much more exalted would be the standing of the general practitioner if people were taught to value his services in diagnosis and his responsibility in counseling surgery and recommending a surgeon until they should expect to pay for this service as they would for any other? It should be the privilege of the surgeon to aid the physician openly to secure this just compensation.

Third—As a rule, the trusting patient is robbed, paying two fees, instead of one, as he supposes.

Fourth—It is the stepping-stone by which incompetent men secure business.

Fifth—It penalizes honesty in the young surgeon who is capable but must sit idly by and starve while his less competent competitor buys the business.

Sixth—It leads to many unnecessary operations with their attendant risk and expense, performed merely because the patient can be induced and has the price. In all, it is betrayal of trust, encourages dishonesty, breeds incompetency, and should, in short, be held a crime. As such, if we expect to retain the standing of our profession, we must brand it with our execration, and drive it out. It is to the credit of the Western Surgical Association that it has made the pursuit of this practice a bar to membership, and has called upon its existing members, if any be guilty, to either reform or to get out. This action of ours has been endorsed by the American Surgical Association. The American Medical Association has passed similar resolutions of disapproval, as have many of the state and county medical societies. The American College of Surgeons has required a signed declaration concerning this matter as a preliminary to application for fellowship. And mark the signs of the times! Wisconsin has passed a law penalizing the offense by revocation of license to practice, with an added fine or imprisonment. Nebraska and several other states have passed similar laws. It has been said that it is a custom which cannot be stopped. I am persuaded that within the limited authority of medical organizations this is true. We can condemn, but we cannot compel evidence; at best, we can but withdraw the privileges and endorsement of membership in our societies. We have condemned. I believe the time has now come when we should as an organization recommend, and, as far as lies in our power, lead in the enactment of laws in every state in the Union which will drive out this practice, which is today the greatest blot on our fair escutcheon. By this method it can be stopped, and

will be, as effectually as has been the rate-rebating of railroads.

In conclusion, I wish to offer two suggestions for added activity of this Association:

First—That an added day of our annual sessions be established as a permanent feature, devoted to clinics conducted by the Fellows of the Association resident at the place of meeting. This is for the purpose of enabling us to become better acquainted with the operative work of our associates. We are all familiar with the clinical ability of surgeons in New York, Boston, Philadelphia, Baltimore, or Chicago, but we know little or nothing of the work equally excellent of our friends in Cincinnati, St. Louis, Minneapolis, Omaha, or Denver.

Second—The Clinical Surgical Society several years ago began the plan of a party trip to some of the foreign clinics every alternate year. These trips have been reported as exceptionally profitable and pleasant. Few of our Fellows belong to the Clinical Surgical Society. Would it not be equally valuable to us if we could inaugurate a similar custom? The itinerary being varied with each successive trip would in time bring to those who went a familiarity with practically all of the work of the world. Such a party would ensure a cordial welcome, and an assurance of systematized preparation with a wealth of material, so that more could be seen with less expenditure of time than by any other method. This plan should stimulate the breadth of our knowledge and, correspondingly, the richness of our work.

THE TREATMENT OF BLOOD-VESSEL INJURIES

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I am encouraged to present this subject for consideration, chiefly because of its seeming neglect and the absence of any approach to uniformity, or the application of scientific surgical principles in the treatment of hemorrhage and its common sequel, shock, by a good many men who are doing surgical work. I wish to acknowledge my indebtedness, in the preparation of this paper, especially to the recent work of Stevenson, "Wounds in War," "The Surgery of the Vascular System" by Burnheim of Johns Hopkins, Lejars' "Wounds and Ruptures of the Great Vessels," and Lexar's "Surgery."

Not a year passes that every active surgeon does not know of the sacrifice of limb or life by an untimely or an unnecessary operation from injury, or from hemorrhage from a damaged blood-vessel, or shock from secondary hemorrhage from not ligating or suturing the injured vessel. Necessarily, the conditions come under the head of urgent surgery, which demands ready wit and prompt and skillful, yet deliberate, action on the part of the surgeon. There is no more severe test of his judgment than the action required to arrest dangerous hemorrhage. In the embarrassment and confusion of the environment and attendant circumstances, the doubtful or wrong thing

is not infrequently done. In military action, 85 per cent of the deaths on the field are due to hemorrhage. ("Wounds in War," p. 117. Stevenson.) The smaller the bullet and the greater its velocity, the cleaner the cut, or wound, of the vessel, whether made at right angles to the vessel or tangentially, therefore the greater, or more profuse, the hemorrhage. If the vessel-wall is grazed or contused, the danger of secondary hemorrhage from sloughing is great. If the coats of the vessel are only partially severed, the hemorrhage is more severe and dangerous than if the entire continuity is divided. In the former, retraction and contraction are inhibited; in the latter, the reverse—the elastic intima, especially, contracting and receding to a greater extent, which is a condition favoring the rapid formation of a thrombus. As shock aids the coagulation, by lowering the blood-pressure and impairing the vascular tonus, its first effect is *conservative,* in the arrest of primary hemorrhage. Lacerated vessels bleed less than clean-cut wounds. Secondary hemorrhage in a clean wound will not occur if both sides of the wounded vessel are ligated or sutured at the seat of the injury primarily. In the establishment of collateral circulation there will be no recurrence of hemorrhage from the distal end of the injured vessel if it is not ligated or sutured. In the medical records of recent wars, the mortality from *ligation* at the site of injury for secondary hemorrhage is much less than from any form of compression or amputation, varying from 43 per cent, in our own Civil War, to 25 per cent, in the Boer War. The Hunterian operation being the more unsuccessful and fatal, the mortality being 36 per cent from secondary hemorrhage, which is not an uncommon sequel to wound-infection, and is more dangerous and difficult to treat successfully under such conditions. Notwithstanding the axiomatic principle that general anes-

thesia and the trauma of operation should not be superadded to the severe shock already existing, it is too often, for different reasons, not respected and recognized in practice. In civil practice the procedure should be different from that which may be perfectly justifiable in military surgery. The better environment and equipment of the former imperatively demands more conservative methods, and, when necessary or advisable, the more elaborate and available technic.

When the caliber of a large artery, with or without its companion vein, of the extremities, is opened by a bullet or sharp instrument, free immediate hemorrhage results. The sharper and smoother the instrument, the greater the hemorrhage. When not immediately fatal, the patient is in severe, if not profound, shock. The first duty of the surgeon is to immobilize the limb, and use the ordinary methods of compression to control hemorrhage temporarily, and then to devote his energies to the promotion of reaction by external heat, salt infusion by rectum, or venous transfusion; and, if loss of blood and severity of shock justify it, direct blood-transfusion as perfected by Crile, and such heart stimulants as one's experience may justify; but it must not be forgotten that the most urgent and necessary heart stimulant is a fair volume of blood in the arteries. The heart-beats will not be quickened nor strengthened unless the vessels are adequately filled with serum or normal blood.

When reaction is established, the second consideration is to prevent an immediate recurrence of the hemorrhage, and also the danger of secondary hemorrhage at a later date, usually from the fifth to the tenth day. Any method of treatment which does not involve, or include, as a necessary requisite, operation

on the damaged vessel itself, suture or ligation at the seat of injury, will be dangerous and frequently fatal to the integrity of life or limb. In a ragged wound the thrombus is more rapidly formed at the site of the injured vessel. It becomes partially organized and temporarily plugs the vessel. As the volume of blood is restored the thrombus cannot be relied upon to resist the *vis a tergo,* or increased heart-beats, until *well* organized. As a result, a clot is generally dislodged, and, in some cases, conclusively indicated by a little fresh arterial blood from the wound within a week, a forerunner of more severe hemorrhage to follow. At this stage, the embarrassments and difficulties are greater than in the primary hemorrhage, and its treatment requires prompt and acute surgical judgment, as well as skillful surgical technic.

Carrel has demonstrated that a circular end-to-end suture of an artery is not followed by either secondary hemorrhage or aneurism. Its success in the human subject has been repeatedly demonstrated. The first arterial suture was by Hallowell, in 1759. Horach was first to try end-to-end suture, in 1888. Abbe succeeded in using a glass bobbin in 1894, which was afterward elaborated by Murphy, with the first circular suture, in 1897, and Jaboulay, Carrel's teacher, and Brian also contributed, while the first arteriovenous anastomosis was by Hubbard, in 1906, for gangrene of the leg.

"If the artery is essential to the life of the part which it supplies, it should be closed by end or lateral suture, depending upon the character of the injury. If not essential, it may be ligated above and below the point of injury." (Lexer: p. 537.) "If the diagnosis of injury to an artery is made, with or without simultaneous injury of a vein, the point of injury

should be exposed, even if there is no external hemorrhage, blood clots removed and the vessel ligated or sutured." (Lexer: p. 555.)

Besides the local and general condition, the application of a stethoscope to the seat of injury will aid in the diagnosis by revealing a blowing sound, or murmur.

An attempt should be made to suture the vessel, in case of ligation, if severe disturbance of nutrition occurs, as frequently happens after ligation of the carotid, more rarely after ligation of the femoral. (Lexer: p. 556.)

Gangrene follows 50 per cent of cases of ligation of the popliteal artery, 20 per cent of the femoral, 50 per cent of both femoral artery and vein, and 1 to 2 per cent of the femoral vein alone. (Lexer: p. 448.) "It must be accepted as a maxim that the preservation of the blood and nerve supply, rather than the condition of the bone and soft tissues, is the most important consideration in the preservation of the limb. Primary amputation is indicated only when the principal vessels and nerves are involved with extensive destruction of bone and soft parts." (Chaumel and Minier, and Stevenson's "Wounds in War.")

The occlusion of the axillary, the brachial, the femoral, and the popliteal may not be considered fatal to the nutrition and preservation of the limb distal to the injured part; but the integrity of these vessels is so necessary and important that the condition should be treated as if it were absolutely essential to the preservation of the limb. The first consideration, after reaction, should be, in case of injury to such vessels, to explore the wound, guided by the track of the bullet, or implement, to the seat of injury, and the vessel treated in accordance with the equipment and technic fully described by Carrel and Burnheim.

(There is a good bibliographical chapter in Binnie's "Operative Surgery.") This is the ideal treatment, but in the absence of the necessary equipment and conditions for its performance, the next step, which should be imperative, is ligation of the vessel, both above and below the point of injury. If the former is done, the blood-supply is restored to the limb, and gangrene will not result, especially in the absence of infection. If ligation is employed, which is the classical method in military surgery, and *heretofore* in civil practice, the preservation of the limb depends upon the establishment of the collateral circulation, and the success of this depends partly on the location of the injury, as well as upon the immediate attendant conditions, general, as well as local. In the majority of cases, the result will be favorable. If infection and gangrene occur in these injuries, the limb distal to the injury will probably be lost. If partial only or localized gangrene occurs, it is still possible to save the limb by suture of the vessels, or reversal of the circulation. If neither suture of the vessels nor ligation is employed, secondary hemorrhage becomes a menace of such momentous importance and danger as to threaten the rest and conscience of the surgeon, for it will come with the suddenness and force of an explosion. Infection increases the probability and danger of secondary hemorrhage. In the absence of infection especially, the patient should have the benefit of the same conservative treatment as in the primary. If infection exists, in the absence of gangrene, ligation is probably the operation of choice; but if the primary injury and hemorrhage are treated in accordance with the principles and technic of modern surgery, secondary hemorrhage will be rare, consequently will not so frequently, as now, demand the consideration of the surgeon. If both artery and vein are injured, the

difficulties and dangers are greater, just as it is when both are involved in aneurism.

In thrombus, with partial or threatened gangrene, reversal of the circulation is promising of better results, and the lateral arteriovenous anastomosis of Burnheim seems to possess advantages over the end-to-end anastomosis of Carrel, with ligation of the distal side of the artery and the proximal side of the vein.

In this paper, it is not my purpose to more than allude to the concealed or intra-abdominal hemorrhage from bullet or stab wounds, nor from the impact of heavy, or falling, bodies on the abdomen. In these injuries, there is not only frequent and dangerous hemorrhage, but the added danger of extravasation of the visceral contents. Such injuries are almost uniformly mortal within forty-eight hours in the absence of *prompt* operation. The mortality from gun-shot wounds of the abdomen in the Crimea was 92 per cent; in our Civil War, 90 per cent; in the Spanish-American War, 72 per cent, and in the Boer War only 30 per cent.

In view of the location of the injury, it is not safe to defer an exploratory operation long in the presence of persistent shock, for the latter indicates continuous hemorrhage or extravasation, or both. But it is to be remembered that when profound anesthesia and the trauma of operation are added to the existing shock, we have made a favorable result not only much more difficult, but often impossible. Under the conditions of such injuries, it is better and wiser as a rule not to employ general anesthesia at all, though full consideration should be given to nitrous oxid and oxygen in preference to others. Local anesthesia may be used to advantage in many such cases; and it should appeal to the surgical judgment in the shock and prostration from primary or secondary hemorrhage.

It sounds the recall of a fundamental principle to protest against its frequent violation in shock and first aid to the injured, and to protest the needless sacrifice, the greater because of the silence of so many teachers and leaders of opinion.

"Shock is exhaustion of the nerve centers." (Crile.)

"Shock is inhibition of the activities of the nerve centers." (Meltzer.)

"Shock is loss of vasomotor control, due to hemorrhage and to afferent peripheral impulses, such as trauma to the visceral peritoneum." (Janeway.)

"Shock is due to a reduction of the dioxid blood-content, and to impaired vascular tonus." (Henderson.)

Regardless of the cause and definition, it is always a serious condition, and a problem of important significance to the surgeon.

CONCLUSIONS

We reaffirm the conclusion, that general anesthesia and operation in grave shock from injury and hemorrhage is indefensible, and that the first effect of shock from hemorrhage is conservative, in promoting the rapid formation of thrombus and arresting bleeding, probably from impaired nerve and vascular tonus in reducing blood-pressure, and depressing the action of the heart. Reaction should be awaited and the interval used in promoting it by well known methods.

If reaction should not occur in a reasonable time, depending on or influenced by the nature of the accident and the previous state of the patient, ether and the trauma of operation would only aggravate the condition and render recovery still more doubtful. If, with reaction, the damaged vessel is not operated on, it will be wise not to stimulate the heart prematurely but rather keep it mildly subdued in order to give sufficient time for the thorough organization of the

thrombus, as, without suture or ligation, it is the chief obstacle to secondary hemorrhage. When reaction is assured, the damaged vessel, such as axillary or brachial, the popliteal or the femoral, should, under local or general anesthesia, be, preferably, sutured, and, secondly, ligated without needless delay. With successful suturing of the vessels, nutrition of the parts is preserved, and gangrene prevented. With ligation the collateral circulation will, in a large proportion of cases, preserve the nutrition and integrity of the parts; but it is more uncertain than suture, yet safer and more reliable than other measures.

Amputation has no place in modern civil practice as a remedy for traumatic hemorrhage, and a smaller place in the military surgery of the future among civilized nations than in wars even of the recent past.

DISCUSSION

Dr. J. Frank Corbett (Minneapolis, Minn.): I have heard the reference made to the Bernheim lateral anastomosis of vessels. In the experimental laboratory I have tried out this procedure a good many times. I found in some instances at the end of five or six days that, when we had turned the arterial stream into a vein, there occurred a severe spontaneous hemorrhage. This was due to dilatation of the vein and ultimate rupture of that structure. I found that I could prevent this unfortunate occurrence by reinforcing the vein and my line of suture with fascia or some other material, so that, I think, it ought to be laid down as a principle, that, where we turn the arterial stream into a vein, we must reinforce the vein with some other structure.

THE USE OF THE WIRE TOURNIQUET IN OPERATIONS FOR GOITER

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The ideal goiter operation should meet certain requirements:

The average surgeon of limited experience should be able to perform it with rapidity, safety and confidence; hemorrhage should be easily and perfectly controlled; there should be no danger of injuring the parathyroid bodies or the recurrent laryngeal nerves; crushing or excessive manipulation of the tissues should be avoided, especially when a tendency to hyperthyroidism exists, and it should be possible to remove with facility as much or as little of one or both lobes as may seem desirable.

The methods in general use fall so far short of this ideal that many otherwise skillful surgeons approach operations upon the thyroid with hesitation, which naturally tends to direct much of the goiter work into the hands of men of special training and experience.

I desire to call attention to an operation, devised several years ago by myself, which in most cases meets the above requirements better than any other procedure with which I am familiar:*

Operation.—The gland is dislocated in the usual manner through a collar incision. This means that the

*Described in a paper read before the Texas State Medical Association, May 9, 1911.

lobe to be removed should be freed bluntly, generally by sweeping a finger around it next to the capsule, and lifted from its bed. This usually can easily be done so that the lobe may be grasped by the fingers, thus putting its attachments on the stretch and forming more or less of a pedicle next the trachea. (Fig. 7.)

Several loops of heavy, braided silk, or, better, of strong fishing-line, are then drawn through the substance of the lobe near its attachment. This may conveniently be done by plunging a small pair of blunt-pointed "alligator forceps" (Fig. 1) through the gland, grasping at its center a ligature a foot long, and dragging the loop into position. Several loops are required: one near each end and one or more in the center, according to the size of the goiter. (Fig. 4.) If more convenient both ends of a loop may be threaded through the eye of a large round needle (Fig. 2), which is then passed blunt-end-first through the glandular tissue; or an ordinary probe with an eye at one end may similarly be used.

As soon as the loops are in place, two wires are adjusted, one on either side of the lobe, and tied tightly in place by means of the ligatures, thus forming a sort of tourniquet which compresses the base of the lobe firmly enough to control the circulation without crushing the tissues. This is easily accomplished by passing one wire through the closed ends of the loops, and upon the opposite side tying the loose ends firmly over the other wire. (Figs, 3, 4, 7.) Additional security may, and usually should, be obtained by again tying the loose ends of the two outer loops around the wires as they project beyond the gland (Figs. 3, 5, 6).

The size of the wire should be about No. 17. It should be capable of being bent without bending too easily. The German-silver wire employed by dentists ("orthodontia wire") answers the purpose admirably.

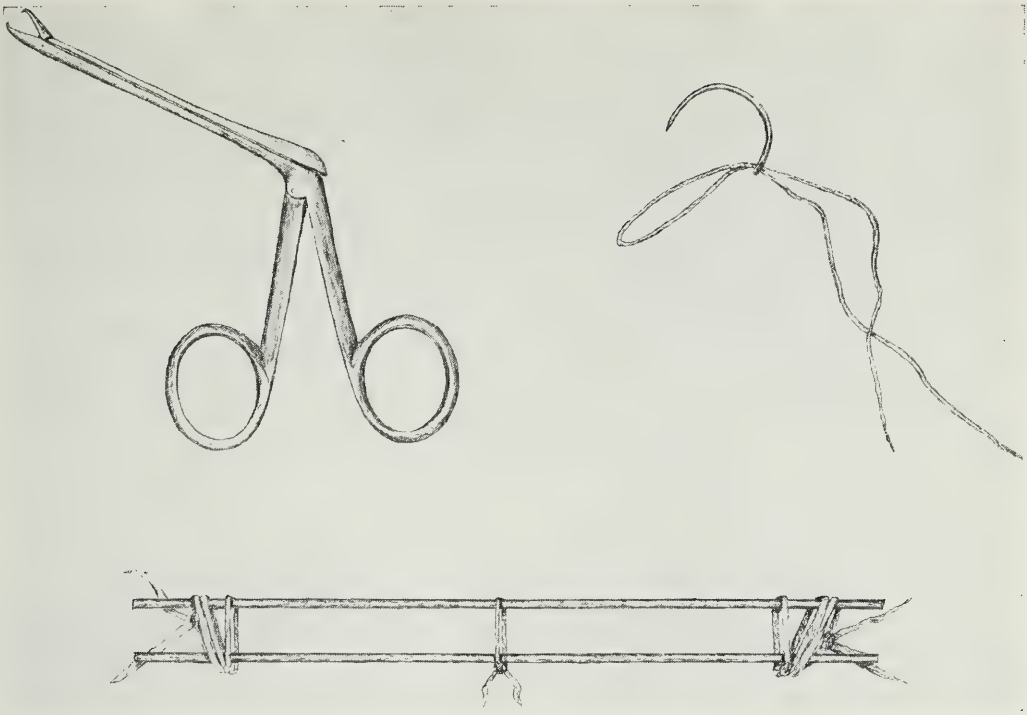

Fig. 1. Fig. 3. Fig. 2.

Fig. 1. Small alligator forceps for pulling loops through gland. Fig. 2. Needle threaded with loop to be inserted blunt end first. Fig. 3. Showing wires held by loops, additional turns being taken at each end.

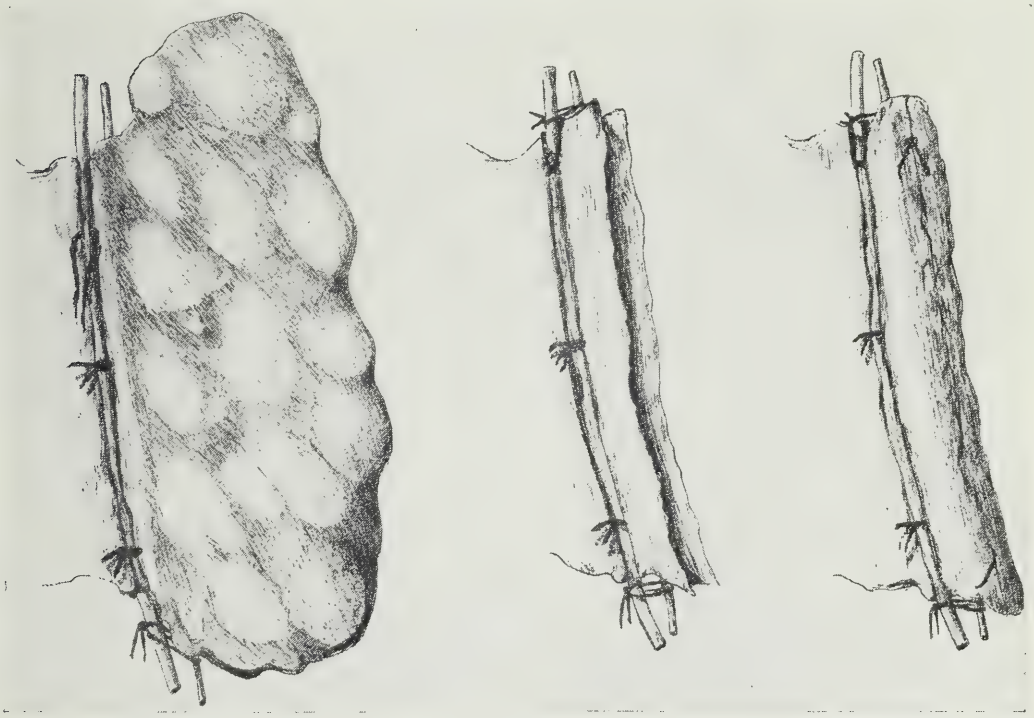

Fig. 4. Fig. 5. Fig. 6.

Fig. 4. Wires in place with loops tired. Fig. 5. Lobe removed leaving grooved pedicle. Note extra turn of end ligatures around extremities of wires. Fig. 6. Raw surface whipped over with running suture.

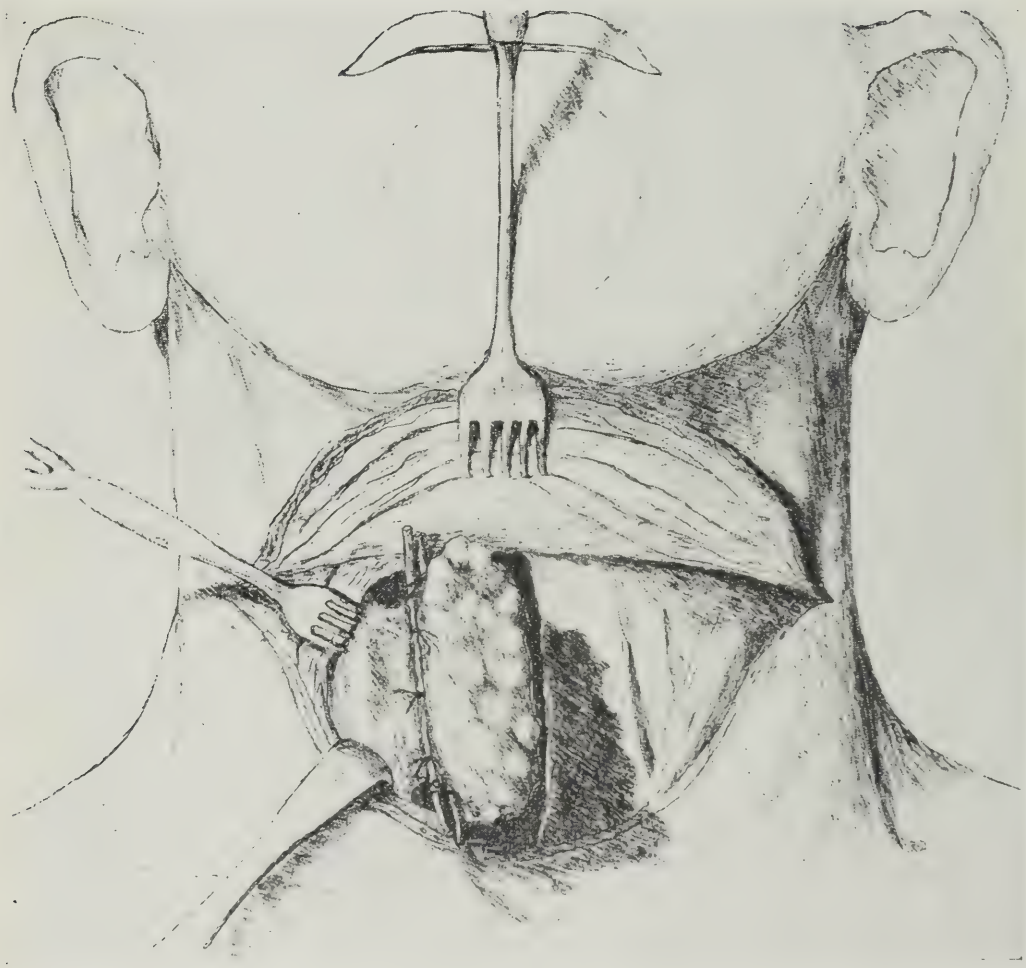

Fig. 7. Showing wire tourniquet in position ready to remove lobe. Note additional turns of end loops around extremities of wires increasing efficiency of tourniquet.

With a pair of cutting-pliers the wires may be cut to appropriate lengths during the operation, and perhaps bent to suit the requirements of special cases.

As soon as the tourniquet is properly applied, which can be done very quickly, the distal portion of the gland is cut away to any desired extent without the least fear of hemorrhage. The wound is next rapidly whipped over-and-over with catgut in a round needle, so as to cover in the raw surface and prevent subsequent bleeding. The application of this hemostatic suture will be facilitated if a sufficient stump is left beyond the wires and especially if the excision is made slightly deeper in the center than at the edge. (Figs. 5, 6.) The wires are then pulled out of the loops, thus freeing the latter so that they may be picked from the tissues; or, if the wires stick, the loops may be cut.

In the majority of instances the procedure goes surprisingly easily and rapidly, and after the gland has once been dislocated it is generally unnecessary to use either artery-forceps or ligatures. Whether or not the superior thyroid arteries are separately tied is largely a matter of choice. I formerly brought the ends of the wires out through the skin-incision, and left them in place for several days before removing them, using, of course, absorbable catgut loops; but I have long since discarded this more cumbersome method in favor of the immediate hemostatic suture, although it might occasionally be useful when the tissues were particularly friable or for other reasons.

The wire tourniquet has a number of advantages:

1. It may be adjusted to almost any goiter, by bending the wires if required, although this is seldom necessary.

2. The control of hemorrhage nearly always is perfect, so that the gland may be sliced off beyond the clamp with absolute confidence.

3. The tissues are not crushed, but simply constricted as in the use of an ordinary tourniquet, which is of importance in Graves' disease at least.

4. There is no danger to the recurrent laryngeal nerves or to the parathyroids. Even if they should be caught in the grasp of the wires, which is unlikely, the compression would not be great enough to do them permanent injury.

5. There is no chance of the wires slipping from the stump after resecting the gland, because they are held in place by the loops passing through the base of the lobe.

6. The wires are much narrower than the blades of a pair of forceps, and hence take up less room.

It may properly be asked why an ordinary pair of forceps cannot be used instead of a wire clamp. There are several reasons: One is that forceps cannot always be applied, owing to the varying characteristics of goiters and the parts which surround them; another is that forceps crush the tissues and may injure the nerves or the parathyroids; while a third objection lies in the fact that when the lobe is cut away the forceps, having nothing to hold them, may slip from the stump and permit embarrassing bleeding.

I have used this method in some twenty goiters of various kinds, where it seemed to be indicated. The operations proceeded easily, safely, without anxiety, and there were no fatalities.

The procedure is particularly adapted to parenchymatous goiters of moderate size and excessive vascularity, such as are often found in Graves' disease. It might be difficult to use in very large goiters, and it would often be unnecessary in the simple enucleation of cysts and tumors. It saves the use of innumerable

hemostatic forceps in many instances, and also the loss of considerable blood, to say nothing of the lessened time and labor and the sense of ease and security obtained.

TREATMENT OF TUBERCULOUS HIP-JOINT DISEASE WITH COEXISTING SINUS BY MEANS OF BISMUTH PASTE, WITH A REPORT OF ONE HUNDRED CASES

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To realize what strides have been made in the treatment of surgical tuberculosis, one must review the medical literature of the past century. The treatment varied according to the views held of the nature of the disease at different periods. Before the era of Pasteur's discoveries the mortality of surgical treatment was most frightful; and surgeons, discouraged, hesitated to operate. According to Follins' figures the mortality at that time due to operations on hip or spinal caries was 56 to 70 per cent. A step forward was made by Lannelongue in France and Koenig in Germany when the study of pathology and asepsis was initiated, and they, with the aid of these, advanced a new form of treatment. We may call this the "daring period." Lannelongue and Koenig advocated the radical resection of the entire tuberculous area. This logical and possibly efficacious practice was no doubt adaptable to smaller foci of disease, but when attempted in the hip or spine it naturally exposed the patient to an enormous risk. This radical form of treatment naturally led also to a large mortality; and, if the patient recovered, he usually remained a cripple

the rest of his life. It was not long before this method also lost its popularity and all its defenders. Less radical methods came into vogue; and now, after many years of diligent study, work, and discussions, the surgeons have up to a certain degree practically agreed on a treatment for surgical tuberculosis.

First of all, they try to diagnose the condition early.

Secondly, they try to check the extent of the disease by immobilization of the diseased joints.

Finally, they place the patients in the best hygienic surroundings, feed them with the most nutritious food, keep them outdoors in the sunshine, and sometimes they add vaccine and tonics to support their general health. Up to this point it seems all surgeons agree, but from this stage on differences of opinion exist. If, in spite of all precautions and the very best of scientific treatments, the case progresses, and abscesses form, what is the proper procedure? The surgeon who will give the correct answer to this question will save many lives, and reduce the big army of cripples.

It is a sad commentary, but it must be spoken, that too many physicians still believe that a fluctuating cold abscess should be opened and drained by a rubber tube. How can we put our words into language strong enough to impress upon them all that this procedure is more dangerous than most any disregard for surgical asepsis? Calot says: "The physician who opens a cold abscess, and drains it with a tube, opens the door through which death nearly always enters." It would be really a wonder if "secondary" infection should not take place. This secondary infection may not kill every patient, and it may be mild enough not even to cause a high degree of fever; but, as a rule, it will cause a persistent sinus and a chronic and disagreeable suppuration. Thus it is certain that cold ab-

scesses should not be incised and drained. What then should be the proper procedure? Should we wait until it ruptures spontaneously? No. It should be treated by either puncture with trocar, allowing some of the fluid to escape and inject a modifying substance (Calot's mixture or formalin, or bismuth paste, etc.). I have used in the past six years bismuth paste (10 parts of bismuth subnitrate and 90 per cent of yellow vaseline) for this purpose, and shall speak of this in the latter part of this paper.

All these methods are fully described in text-books. The results reported are most gratifying, providing the technic is properly carried out.

If, in spite of all this, a secondary infection should take place, and a chronic suppuration should persist, then I recommend the injection of bismuth paste; and to this form of treatment I shall devote the larger part of my paper.

It is now about eight years since this method was introduced into surgery. From time to time I have reported the progress made in its application, in order that others might become better acquainted with the technic, its limitations and dangers.

The method has from the start found many adherers, and has been tried, not only in hospitals and clinics here and abroad, but has been used most extensively by country practitioners. Reports from these sources have already accumulated in the literature so that at the present time one may draw conclusions from them.

Some authors have obtained better results than we have, others have been only partially successful, and in the hands of a few the method has been a failure. If we take into account, however, that we deal with a class of cases which have already been treated by other methods, surgical and otherwise, without success,

we must regard even the smallest percentage of cures an actual gain. The analysis of reports from all sources, however, indicates that on the average more than 50 per cent of these apparently hopeless cases were finally cured by these injections.

At one time it received a serious setback by reports of cases of fatal bismuth poisoning, which, I am glad to say, at present occurs very rarely, considering that the method is now used more extensively than ever. The prompt reports of these cases of poisoning, and several papers which I published for the purpose of its prevention, seem to have been effective in checking further occurrence of this complication, inasmuch as only one case has been reported since 1912.

Many of you have no doubt had experience of your own in treating sinuses resulting from empyema or joint affections with these bismuth injections; and you are familiar with the method from the literature. This relieves me in a degree of the necessity of discussing the theoretical side of this subject, and permits my time to be devoted to the practical side.

I have decided to report only on one type of this class of cases, and have intentionally chosen the tuberculous hip-joint disease, because it is the type which is the most unfavorable for treatment, which has the largest mortality, and in which naturally the percentage of cures was the smallest.

Most of the cases of hip-joint disease which have come to us for treatment are those which years ago had passed through the acute stage, and developed abscesses, which, as a rule, had been drained and secondarily infected. You are familiar with such cases. They constitute the worst scum of surgical refuse, being cases in which every sect of medicine and surgery, and even quackery, has had a chance. Two of these cases in our series had lasted forty years,

and many had had from ten to fifteen surgical operations.

Such surgical residuum is not a very inviting field for anyone; and, I am sure, a discriminating surgeon will not envy me for this class of material, nevertheless out of these apparently hopeless subjects, I am able to exhibit many most satisfactory results. My purpose, however, does not consist in showing you what results I personally can obtain with this method. You, after all, are interested only in what you yourselves might accomplish; and therefore, instead of citing percentages of cures, I shall select from the series of 102 thus far treated only such cases as will suggest new points in the treatment, and aid you in applying the method effectively. The histories of all cases will be reserved for a future publication in connection with complete report of all varieties of cases treated.

The Bismuth Paste Has Been Employed for Diagnostic and for Therapeutic Purposes, as Well as for the Treatment of Cold Abscesses.

FOR DIAGNOSTIC PURPOSES

Its usefulness for diagnostic purposes has been acknowledged by all who have employed it. Medical literature from all parts of the world contains records of hundreds of cases in which this method cleared up puzzling conditions. To illustrate, I cite two cases which were supposed to have been hip-joint disease, but which in fact were sacral tuberculosis.

TUBERCULOSIS OF THE SACRUM MISTAKEN FOR HIP-JOINT DISEASE, TREATED AND OPERATED ON AS SUCH

Case 39.—Wm. E. E., aged 25, single, was treated for rheumatism of the hip from 1902 to 1904. In 1904 he suddenly had a pain in his right groin; and a few days later a swelling appeared underneath Poupart's ligament. Two

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months later this was incised, pus evacuated, and a rubber tube fourteen inches long was inserted. Following this, he became very much emaciated, and chills and fever persisted. For two years he was treated for hip-joint disease because a contracture of the adductors and an impaired motion of the joint misled the doctor, and no radiograph had been taken at the time. This operation aggravated matters; and six months later another operation was performed with no other results than adding another sinus above the posterior part of the crest of the ilium. Altogether four sinuses resulted which kept on suppurating constantly, but his health gradually improved. He came to me in 1910; and the injection of these sinuses cleared up the mistake in diagnosis: namely, that this was not a hip-joint disease, but a tuberculosis of the sacrum. The injections of bismuth were continued for three years. While his general health was improved and his weight increased, the sinuses persisted in discharging about the same quantity of pus. A sequestrum was localized in the promontory of the sacrum, and I decided to remove it. This was done three months ago; and upon the removal of the sequestrum all four sinuses closed. Unfortunately, two months later he developed an abscess in the lumbar region on the other side, which I have opened only ten days ago, and removed another sequestrum from the sacro-iliac joint on the left side. The final outcome is problematical.

Case 100.—F. F., age 23, had an attack, supposed to be rheumatism in the sacral region, had high temperature, and was confined to his bed for six weeks. He was then apparently well until the fall of 1912, when the hip on the left side became double the size of that of the right. Osteopathic treatment was applied; and later the hip was lanced by the osteopath in the region of the upper gluteal region, and a discharging sinus persisted. Three months later two abscesses formed on the posterior aspect of his thigh, which were operated upon, and then two sinuses were added. Bismuth injections were used by his physician without any results. He came to me in November, 1913, for examination. An injection into the sinuses cleared up the diagnosis. Instead of the suspected hip-joint disease, it proved to be a sacro-iliac tuberculosis, as our illustration clearly shows.

Examples like this explain to a degree why surgical operations for these chronic suppurating sinuses often fail. A glance at the radiograph, which represents in-

jected sinuses, teaches us how irrational it is to attempt a dissection of tortuous sinuses which lead into an inaccessible region. In the light of these pictures the probing of a sinus will not appeal to those who wish to be consistent.

THERAPEUTIC PURPOSES

At the present time very few doubt the therapeutic effect of the bismuth paste in old chronic sinuses and empyema.

In our own series of 1,100 cases treated at the North Chicago Hospital we have used the paste in practically every variety of chronic suppuration, such as sinuses resulting from spondylitis, hip-joint disease, and tuberculous knee, ankle, shoulder, wrist, and ribs. Osteomyelitis in all parts of the osseous system, including the clavicle and fibula, is extremely rare. Further, in chronic suppurations of soft structures, such as sinuses after extirpation of kidney, broken-down tuberculous glands, rectal fistulæ, and sinuses following laparotomies, this method has been used. The accessory sinuses of the head and fistulæ of the alveolar process, have also been treated by this method. Only fistulæ of the gall-bladder, the pancreas, and those communicating with the cranial cavity have, for obvious reasons, not been treated.

It is quite difficult to obtain statistics as to the extent of the use of this method and the various surgical conditions in which it has been applied, because the bulk of the cases come into the care of the general practitioner in the country, who treats them in his office or the patient's home, and thus only a small fraction of treated cases find their way into literature. Up to 1913 the following have made reports:

Name	No. of cases		Percentage of cures
Ochsner, Chicago...	20	Tubercular sinus..	55%
Ridlon & Blanchard.	17	Tubercular sinus..	53%
Beck, E. G., Chicago	192	Collective report..	64%
Robitschek, Minneapolis	9	Tubercular sinus..	55%
Don, Edinburgh....		Tubercular sinus..	17%
Rosenbach, Berlin..	4	Tubercular sinus..	50%
Dollinger, Budapest.	16	Tubercular sinus..	12½%
Beck, J. C., Chicago.	319	Accessory sinuses.	22%
Pennington, Chicago	17	Rectal fistulæ.....	76%
Baer, Baltimore....	12	Tubercular sinus..	33⅓%
Stern, Cleveland....	4	Tubercular sinus..	100%
Steinman, Muenchen	5	Tubercular sinus..	20%
Bogardus, U. S. A..	1	Tubercular sinus..	100%
Vidakovich, Russia..	2	Empyema	100%
Nemanoff, St. Petersburg	6	Empyema	100%
Ochsner, A. J., Chicago	14	Empyema	85%
Beck, E. G., Chicago	11	Empyema	82%
Ely, New York.....	14	Tubercular sinus..	43%
Hines, Cincinnati...	9	Tubercular sinus..	89%
Cuthbertson, Chicago	1	Intestinal fistula ..	100%
Sandov, Sg., Budapest	2	Otologii	100%
Heitz, Boyer Morens, Paris	11	Renal sinuses.....	73%
Zollings, Zurich....	25	Tubercular sinus..	54%
Schober, Philadelphia	5	Tubercular sinus..	80%
Gessner, New Orleans	4	Tubercular sinus..	50%
Schmid, Vienna....	15	Tubercular sinus..	30%
Rivera, Porto Rico..	8	Tubercular sinus..	75%
Goror, E., Paris....	3	Tubercular empyema	66%

Name	No. of cases		Percentage of cures
Reichelfelder, Wash.,	4	Tubercular empyema	75%
Brandes, Kiel.....	28	All varieties of sinuses	76%
Beck, R., Chicago...	58		
Beck, R., Chicago...	9		
Collective reports from 19 dental surgeons in U.S.A.	4	Alveolar sinuses..	54%
		Empyema antrum.	100%

We note that the results are not the same in the hands of the different reporters. I believe I can explain the reason for the unequal results. During my visits to various hospitals in America and Europe I have had the chance to observe why some men have failed. I shall name some of the reasons.

My brothers and I have treated a group of cases in which the bismuth paste had been applied by others without success. This furnished the opportunity to study the causes of failure in a variety of most instructive cases. In some of the cases the cause of failure could not be explained. A case often healed after our first injection, whereas the same patient had been treated elsewhere by the same method without success. We could not tell whether faulty technic, unsuitable instruments, or improper material was responsible for the failure. In other cases, the causes of failure were quite apparent. Aside from the most common cause, namely, the presence of a sequestrum, we found foreign bodies. In one case, for example, a metal probe in the humerus; in another, a rubber tubing within the medullary canal of the humerus, accidentally left in years before; then, two rubber tubes within an old drained empyema cavity; etc. These were causes which had prevented the paste from

obliterating the suppuration. As soon as these foreign bodies were removed, the cure was almost spontaneous.

The most common cause, however, is the faulty technic and insufficient knowledge of the rules which have been laid down for the treatment. "How often do you inject?" I am asked very frequently. The answer to this question is: "As often as you have failed to reach the focus of the disease."

We must remember that a sinus or fistula is nothing more than a shriveled abscess or abscesses. It leads from its opening on the skin or bowel to the place where the disease originated, and this focus of disease is often at a considerable distance from the opening or openings of the sinus. It is therefore inconsistent to try to eradicate the suppuration by dissecting only the sinus tracts. With the radiographic reproductions of the labyrinths of sinuses before us, an attempt to dissect the same borders on the ridiculous.

To find the root of the trouble and to eradicate it, is the only rational method to pursue. If you find the focus from which the sinus originated and disinfect it, in practically all instances the sinuses will close up.

It is therefore essential that, when a fistula or sinus is injected with bismuth paste, the paste must reach the focus of the disease. If through faulty technic this is not done, one cannot expect results.

The first injection ought, therefore, to produce the desired result. If it does not, then we must assume that the paste has not found its way into all portions of the diseased tract, and we must try it again. It is a safe rule to wait at least one week. If the discharge changes its character from a purulent into a serous, and the microscopic examinations of a slide and culture show that the secretion is sterile, we should not re-inject, since the sinus will heal within a very

short period. If, however, the discharge continues to be purulent, and we continue to find micro-örganisms in it, then we should re-inject at least two times a week. If there is no change within a reasonable length of time, then we should search for the cause of failure.

Most of these points naturally developed with the evolution of this treatment, and through application of the same method to a large series of one class of cases. The hip-joint disease offers a greater opportunity for study than any other form; and I wish to relate what we have learned from treating a series of 102 consecutive cases.

We have made the following general observations:

First—We have found that hip-joint disease in its incipient stage is too frequently diagnosed as rheumatism. Two-thirds of our cases gave the history of having been treated in the beginning for rheumatism or sciatica.

Second—We have further noted that when the disease had progressed to the stage of abscess formation, incision and drainage was, as a rule, the form of treatment, a method which, of course, is now generally condemned.

Third—We noted also that sacral tuberculosis, or that of the sacro-iliac joint, is frequently taken for hip-joint disease. We have met with five such cases. The reason for this mistake is explained by the similarity of the swelling and contracture of the limb on the affected side, which we find in hip-joint cases. The radiogram in such cases is a deciding factor. This aid had been used in only two of these five cases previous to our examinations; and in these the pictures were not clear enough to make their interpretation possible. Here I cite an example.

TUBERCULOSIS OF SACRUM MISTAKEN AND TREATED FOR HIP-JOINT DISEASE

R. K., aged 13, with a tuberculous family history, was well until the age of five, when a swelling in his left hip, posterior to the greater trochanter, appeared. An abscess ruptured spontaneously, and a copious purulent discharge persisted for seven years, which greatly debilitated the boy. In July, 1908, he was given the first injection of bismuth paste by Dr. Dahl, with whom I treated the case. The radiograph disclosed the fact that the abscess did not originate in the hip-joint, as one would suspect from the location of the sinus, but it communicated with the original focus in the sacrum by a narrow channel. This case not only proved to be an interesting example of the diagnostic value of the bismuth paste, but it likewise credited the therapeutic account with a cure of which both the doctor and patient are justly proud.

Fourth—Another rather rare complication occurred in four of our cases; namely, that the hip-joint disease produced a rectal fistula. I illustrate all four cases, but give only short histories of each. These belong, of course, to the most severe type of joint disease, in which the abscess has spread along the path of the fascia, usually through the notch below the anterior superior spine, then along the pelvic fascia toward the pararectal tissues, and there either opened into the rectum or around the anal opening.

HIP-JOINT DISEASE CAUSING INTRARECTAL FISTULÆ HIP-JOINT DISEASE AND SPINAL TUBERCULOSIS CAUSING RECTAL AND ANAL FISTULÆ

Case 12.—T. S., a young lady, aged 17, developed a typical hip-joint disease two years previously. After extensive destruction of the joint and shortening, two abscesses formed, and one opened above Poupart's ligament, the other on the anterior aspect of the thigh. She complained also of intrapelvic pain, constipation, and pain during urination. An examination of the rectum revealed an abscess high up in the rectum. Compressing the abscess would cause the escape of pus through both sinuses in her groin. The stereoradiograph of the bismuth paste gives a very graphic picture of the

abscess formation in the vicinity of the rectum. The abscess ruptured spontaneously into the rectum about three weeks later, and resulted in an intrarectal fistula. The final result in this case was a complete cure, of the rectal sinus as well as the hip sinuses; and aside from the shortening of the limb and impairment of motion the patient is entirely well.

Case 50.—I. B., aged 36, developed hip-joint and spinal tuberculosis at the age of nine years. After a number of years of conservative treatment, abscesses formed around both hips and lumbar region. Several of these were incised, and some opened spontaneously, so that at the age of 20 she had ten discharging sinuses about the hips and back. During the next ten years she had undergone a number of surgical operations, none of which was of any avail. At the age of 28 she joined the Christian Scientists and became a most devoted disciple of that cult, receiving their constant treatment for eight years. I saw her first in 1909. She could not walk, but could manage to get out of her very low bed to urinate about every half hour, on account of a chronic cystitis.

This cystitis was the result of a perforation of a sinus into the bladder. One sinus perforated into the rectum, and another about one inch from the anal opening. Here, then, we have a combination of a hip and spinal tuberculosis causing a rectal as well as a vesical fistula, a most distressing combination. The bismuth treatment had the effect of closing the rectal as well as the urinary fistula, but several of the other sinuses continued to discharge. The patient was soon able to be up and walk for blocks and she resumed her worship at the nearest Christian Science church.

In all sincerity she attributed her improvement to her faith in Christian Science. In order not to hurt my feelings she declared that I myself was a Christian Scientist, but was not aware of the fact. The final result I could not trace.

HIP-JOINT DISEASE CAUSING ANAL FISTULA

Case 58.—E. D., aged 11. Five months ago he had a severe chill, a fever, and a severe pain in the hip for three weeks. Abscess formed, and ruptured one week later. Another abscess opened between the scrotum and anus ten days later. These kept on discharging profusely until he came to me in October, 1911. He was in an emaciated condition,

hardly able to turn in bed, weighing only 53 pounds. The accompanying illustration, the radiogram of the injected sinuses (Fig. 6), gives more information than any description. We note the course of the fistula into a pararectal abscess, and from this a narrow sinus leads along the pelvic fascia up to the hip-joint, where it fills a cavity left by the destroyed head of the femur. Another abscess gravitates down along the fascia lata. This represents a very acute form of tubercular and mixed infection, which, as a rule, is fatal. This case, however, improved upon the very first injection. He gained in weight at the rate of three pounds a week and today he walks.

HIP-JOINT DISEASE CAUSING RECTAL AND ANAL FISTULA

Case 64.—Young man aged 28, developed at the age of eight a hip-point disease with an extensive destruction of the hip-joint, consequent abscesses about the hip, and one near the anal opening. For the past twenty years six sinuses kept on discharging profusely. I first saw him in 1911. He had a shortening of the left leg of four inches, and all six sinuses discharged so profusely that it required two dressings each day in order to preserve any semblance of cleanliness. Repeated injections of bismuth paste produced considerable improvement,—two sinuses closed, but the remaining four continued to discharge moderate quantities of green pus.

In May, 1913, he developed another painful abscess near the rectum. I incised and injected the same and found the network of sinuses and their communication with the hip-joint, as shown in the radiogram. Since the last injection the patient gained fourteen pounds, but four sinuses still remain open. He reports that from time to time small pieces of bone are eliminated from the sinuses.

The following case represents a more uncommon complication:

TUBERCULOUS SPONDYLITIS WITH SIXTEEN URINARY FISTULÆ

Case 40.—J. G., aged 25, suffered at the age of seven with pain in his right knee. A year later an abscess formed over the great trochanter, and shortly after more abscesses formed around the whole pelvis, all of which opened and the final result was twelve sinuses, all of them discharging pus mixture with urine.

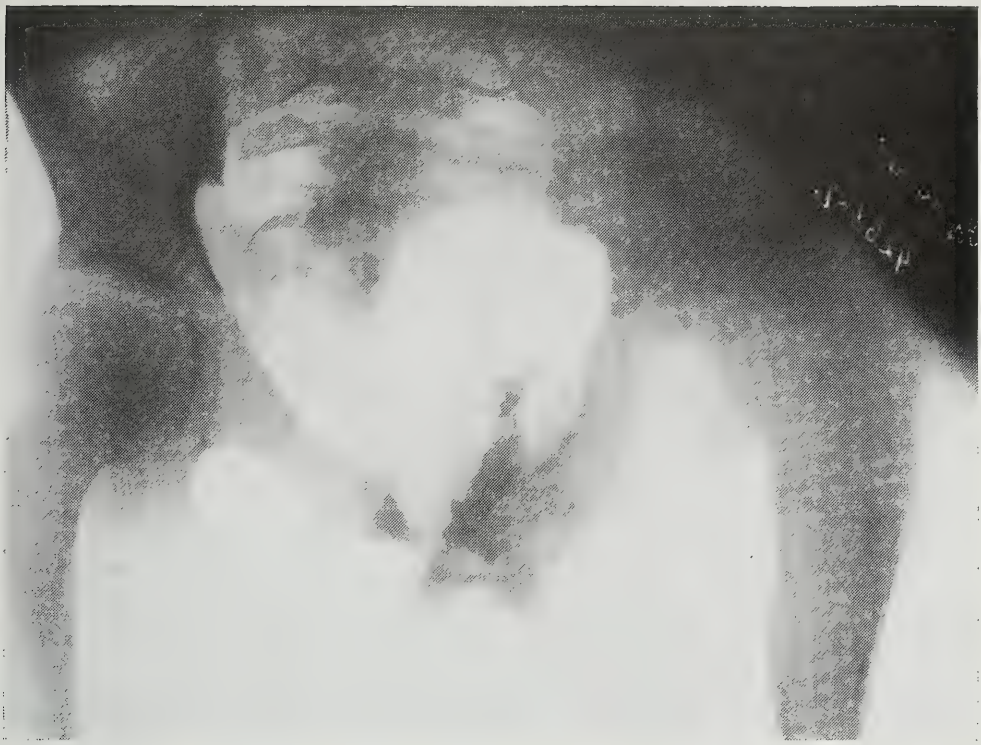


Fig. 6. Rectal fistula originating from hip-joint disease.

Fig. 10. Tuberculous hip, with destruction of the iliac bone.

For the last seven years not a drop of urine was passed through the urethra. In 1906 Dr. Steele of St. Louis curetted several of these sinuses. New abscesses formed so that when he was referred to me he had sixteen urinary sinuses, his right leg was flexed, and his hip ankylosed. The entire pelvic region was in an acute eczematous condition from the constant saturation of urine and pus.

I injected all fistulæ at once with a 33 per cent bismuth vaseline paste, and two hours later, for the first time in seven years, the patient passed his urine through the natural channel. The urine, however, contained a large percentage of pus and some of the paste.

The radiogram (Fig. 8) exhibits a veritable labyrinth of sinuses.

The sinuses began to close rapidly, and the eczema was markedly improved; but a new trouble started. The patient developed a severe cystitis which required frequent irrigation of his bladder. Within two weeks eight sinuses were closed, the others markedly improved, and only one discharged a few drops of urine. In this condition he left my care and returned to St. Louis. The final outcome is not known to me.

Another unusual case is the following:

HIP-JOINT DISEASE REQUIRING REMOVAL OF THE ENTIRE FEMUR

A. V., aged 32. His illness dates back twelve years. It started with a severe pain in his right knee. In this case hip-joint disease was at once diagnosed, and the limb immobilized. Six months later he had to submit to an operation on his hip, and to another six months later, and all wounds healed primarily. For five years all seemed well, when, all at once, abscesses appeared around the hip, and this time the head of the femur and its upper one-fourth were removed. The resulting sinuses would not heal in spite of persistent treatment for five years, and thus another operation for the removal of the entire femur, down to the condyles was performed. All healed nicely except a small fistula in the groin remained. In November, 1908, he was first examined by me and I found that although the entire femur from the condyles upwards was absent there was not so marked a shortening as in other similar cases. The limb could be flexed on itself in the center of the thigh to a right angle. Although he

was told that his limb would be useless without bony support he would not permit its amputation, his reason being that some day surgery will be so advanced as to be able to supplant a bone and make the limb useful. The radiogram illustrates the condition present. The two parallel narrow strips running upwards are regenerated strips of bone where some periosteum had been retained in the muscular channel surrounding the femur. The sinus leads into the cavity of the acetabulum. I have been unable to close this little fistula and for this reason hesitate to advise the transplantation of a femur until we succeed in closing the sinus.

Fifth—The acetabulum. How often is it affected?

The acetabulum is affected in the majority of hip-joint cases. Rarely does it break through into the pelvis, and allow the abscess to find its way into the pelvis; but it does sometimes happen. We have one such case on record. (See report of four cases of rectal fistula.) The iliac bone is affected in about 5 per cent of the cases.

I cite here a case in which the larger portion of the iliac bone was destroyed.

Case 90.—Miss M., aged 16, developed hip-joint disease at the age of six. The process of destruction was so extensive and rapid that it destroyed the entire head of the femur, and soon involved the entire ischium of the pelvic bone. The destruction of the latter bone was so extensive that after a few years there was only the central part of that bone left. The outer margin of the iliac bone for about one and one-half inches was destroyed, leaving only a ragged edge on its circumference. An abscess formed about six months after the incipient trouble, and three sinuses resulted after the rupture and incision of the same. There was a persistent discharge for nearly ten years. The girl could not walk without crutches, and was in an emaciated condition. In April, 1913, the sinuses were injected, and it was proved that they all communicated, and that within the acetabulum there was a small cavity, indicating that sequestra must be present. (Fig. 10.) I removed these sequestra by curetting through one of the sinuses; and subsequent injections produced a closure of all sinuses within a month. The girl gained rapidly in weight and is able to walk without crutches.

A case remarkable from many points of view is the following:

HIP-JOINT DISEASE WITH CO-EXISTING PULMONARY TUBERCULOSIS

Case 81.—F. B., aged 17, was taken with pulmonary tuberculosis when fifteen years old. The affection was quite acute, and caused a rapid decline in her health. To this was added a hip-joint disease. For one year the hip was put in a cast; and she was placed in the most favorable surroundings to restore her health, and was in charge of the best medical counsel. In spite of all this, an abscess formed and was allowed to rupture spontaneously. Secondary infection took place, and all that she had gained during the past years was lost, her weight having fallen from 125 pounds to 89 pounds. In this condition she was brought for treatment. The tuberculosis of her lung was still present, but from her description was somewhat improved.

There were two sinuses about three inches apart, both in line of the Poupart's ligament. These sinuses discharged about two ounces of green pus every twenty-four hours. The injection of the paste and the radiograph disclosed a most interesting condition, namely that these two sinus openings, while very near to one another, led in two different directions. One led to a coil of sinuses within the pelvis underneath the iliac fascia, while the other led into an abscess along the fascia lata. This demonstrated to us that no matter how near together two openings of sinuses are, they do not necessarily communicate.

In the absence of the radiographic guide one would be tempted to unite these two openings when operating, which, of course, would be entirely wrong.

The therapeutic result in this case was most unusual. The pus discharge changed into a serous within twenty-four hours; and from this time on the patient gained at the rate of from four to eleven and one-half pounds each week until she had gained forty-eight pounds within two months. Both sinuses closed, and, what was more remarkable, she had lost all signs of her pulmonary tuberculosis. Last November, two years after the closure of the sinuses, I invited her to appear before the Surgical Congress in Chicago, and to my surprise she came with her husband, having married in the meantime.

This case is instructive for many reasons.

It is rare that active pulmonary disease co-exists with bony tuberculosis. In my experience of about 450 cases of tuberculous joint disease, I remember but three cases in which I could find an extensive co-existing pulmonary affection. I have found evidence of a healed-out tuberculosis of the lung in some of the cases by taking stereoscopic radiograms of their chests, but the active type is rare; in fact, the joint tuberculosis seems to protect one against the pulmonary. Why did the pulmonary tuberculosis cease when the local condition in the hip was cured? This is a large subject; and I cannot go into it in this paper, but I have sufficient data, clinical as well as experimental, to offer an explanation, which I shall report in the near future.

These cases cited are, of course, the more unusual types. The bulk of the hip-joint cases where they have reached the point of sinus-formation have one to five inches shortening, a tipped pelvis, and three to four sinuses. The openings are usually distributed as follows: one just below Poupart's ligament internal to the iliac vessels, another along the external border of the gluteus maximus, and a third just below the greater trochanter. Once in a while another sinus opens near the sacro-iliac synchondrosis.

We have noted that a patient, who for years could not carry his weight on the diseased limb on account of the acute pain in the joint, and was obliged to use crutches, could, after the joint was closed, walk without pain; and we have, in most instances, allowed the patients to discard the crutches. In 1909 I showed at the North Side Branch of the Chicago Medical Society four such children in whom I had recently closed suppurating sinuses, and allowed them to walk without crutches. I maintain that after several years'

standing the tuberculous focus had been extinguished, and that the secondary infection was all that the child was affected with. In none of these four cases has there been any recurrence.

THE PREVENTION OF SINUSES

This procedure consists in the opening of cold abscesses and injecting them at once with a 10 per cent bismuth paste without suturing the opening or introducing a drain. The quantity used depends upon the size of the abscess, but should not exceed 100 grams, because in these fresh abscess walls absorption of bismuth is apt to take place more rapidly and cause bismuth poisoning.

We have treated with this method 110 cases, of which three are of the hip, and we have made the following observation:

A. That in practically all cases we could prevent a secondary infection.

B. That the creamy pus upon opening the abscess was changed into a straw-colored fluid within three or four days after injection.

C. That 90 per cent of all cases thus treated closed within three weeks after incision and injection.

The preference to the bismuth to other modifying substances was given for the following reasons:

1. That paste is injected through a small incision, instead of using a trocar, and thus the possibility of missing the abscess is eliminated.

2. By discarding the aspirating-needle the danger of injuring underlying vital structures is avoided.

3. Through an incision it is possible to evacuate the larger clumps of the tuberculous débris, which could not pass through the aspirating-needle.

4. The thick paste within the cavity will allow the escape of secretions along the walls of the abscess, but secondary infection is prevented.

5. Injections of other modifying fluids must, as a rule, be repeated, while with the paste the first injection usually attains the desired result.

6. The injection of bismuth paste is not painful or irritating. It is injected in a warm semiliquid state, and remains long enough in contact with the diseased tissues to produce its therapeutic effect. The vehicle (vaselin) does not macerate the walls of the abscess. Toxic effects from bismuth subnitrate can easily be prevented.

DANGERS AND COMPLICATIONS

The only danger which has been advanced is the possibility of bismuth poisoning. My brothers and myself are fortunate in not having had a single fatal case in our series of 1,100 cases. I met with the symptoms in one case of empyema treated during the infancy of this treatment; but I was able to check the progress and save the patient. This case was reported by me in the *Jour. of the A. M. A.,* January 8, 1909, and is the first case on record. I warned the profession then against the indiscriminate use of the paste. It is fortunate that most of these accidents were at once reported in the literature, as this has put on guard those who thought that bismuth was an entirely harmless substance. It must have had a very salutary effect, because nearly all the cases of poisoning occurred in the first two years 1908 and 1909.

It is gratifying to know that the poisoning can be checked before it causes irreparable damage.

PREVENTION OF BISMUTH POISONING

The prevention consists of not allowing large quantities of the paste to remain in the body for absorption.

Should the symptoms appear, the paste must be removed by washing out the cavity with warm olive oil. The sterile oil is injected, and retained from twelve to twenty-four hours, in order to produce an emulsion, which should be withdrawn by means of suction. After its removal all symptoms will promptly disappear. Scraping out the paste with a scoop is a dangerous procedure, because it opens fresh channels for absorption.

DISCUSSION

Dr. James Winfield Cokenower (Des Moines, Iowa): The modern aspect of anatomical structure of sinuses convinces us that we have many foci to contend with; and as these abcesses are cold and the contents about nine times out of ten sterile, it would seem that opening them wide and curettage would not be indicated.

The author of the paper read has just kindly told you what to do to be successful. Hence, I will tell you what I did that was not successful and why I did not succeed. I had tried carbolic acid, formaldehyde, iodoform, and everything that was along the line of treatment of injections, and I failed. Then when this bismuth paste came to our notice I tried it, and failed; and I not only failed, but got bismuth-paste poisoning. The reason for the poisoning was, that I made too large an incision in a cold abscess. There seemed to be considerable necrotic tissue, so I curetted, but did too much and exposed new tissue, after which I injected the paste; and what was the result? In the place which I left raw the paste was taken up by absorption, and poisoning was the result. Then, to obviate my mistake, I injected olive oil, filled the wound up solid, let it remain for twenty-four hours, and then pumped out my paste, and saved my patient.

Another way that I have failed, was because of the sinuses being tortuous, and oftentimes I failed to permeate many of them to the terminals. Dr. Beck himself cannot make a success of the treatment, if there is a sequestrum or any other foreign body at the nidus of the disease, and whenever that occurs you have got to cut down and remove the cause. Not all of the cases have this, but when they do you have got to use surgery. The best plan is to make only a small incision, inject it with bismuth paste, and then the presence of that will

stimulate leucocytosis, dispense with the phagocytes, and you have a proliferation of tissue; and, if the injection has been properly done, one injection is sufficient; otherwise, not. Whenever I undertake to repeat I always feel that I have made a mistake in my first effort, because, if I had not done so, it would have been successful.

Dr. William T. Reynolds (Kansas City, Mo.): I would like to ask Dr. Beck to explain the process of repair. What takes place, what brings about the cure?

Dr. Beck (closing): I am afraid the gentleman will call me down for time if I begin to discuss that subject. I think I could not explain it in two minutes, or in five, but had better refer you to the literature, which is extensive. Johns Hopkins University investigated it. I believe, from observation, it is the leucocytosis which is underlying, and this is probably generated from the subnitrate of bismuth. It may be that it is not absolutely necessary to use bismuth—there are other substances that might do—but, so far, I have found no good substitute. I have tried fifteen substances, but none have proved so satisfactory.

REPORT OF INTERESTING BONE CASES

By William Hessert, M. D., F. A. C. S.

CHICAGO, ILLINOIS.

Case 1.—Male, aged 36 years, single.

Family history, negative. Parents, brothers and sisters living and well. No history of fractures in the family.

Venereal history, negative.

Previous history: Patient was an eight-months' baby, and was always frail and delicate. His mother raised him on the bottle, and states that from the beginning he suffered greatly with gastro-intestinal trouble. The eruption of teeth was late, and he was very tardy about learning to walk. In his early infancy he fell and sustained a fracture about the right elbow. Extension and flexion of that joint have been limited ever since. He was small and dwarfed as he grew into boyhood, and was backward mentally.

When nine years of age he fell, and fractured his right-femur shaft. The next year the same bone was broken again. At fourteen he fractured the shaft of the left femur. His fifteenth year was an unlucky one; for on two occasions he sustained fractures of both femora, and once the right femur alone was broken. On account of slow union, he was laid up the greater part of the year nursing his fractures. From fifteen to twenty he had no trouble; for he had learned to be extremely cautious about jarring his frame.

He became a cigar-maker, and led a very quiet and

abstemious life, indulging very moderately in beer and tobacco. At the age of twenty he had another fall; and, striking on his hip, he sustained a fracture of the neck of the right femur. He was completely disabled for many months, and since that time has walked with a decided limp, and has had to use a cane.

Present trouble: When 35 years of age, while walking on the sidewalk after a rain, his lame right leg slipped from under him, and he went down on his right side. The right arm, which was held out to ease the fall, received the entire weight of the body. He recovered his feet, and suffered only moderate pain in the injured member, but realized, from its uselessness, that it was broken.

Examination: The patient presents in marked degree the sequelæ of early severe rickets. He is a typical rickety dwarf, about four feet in height, and weighs 140 pounds. His mental condition is fairly bright, and the voice boyish and high-pitched. The head is large and square, with receding forehead, and there is slight exophthalmos, but no indication of Graefe's, Moebius', or Stellwag's sign. The thyroid is rather small and firm and does not pulsate. The chest is broad and deep, and rather asymmetrical; and the spine shows lateral and anterior curvature. The shoulders are very sloping, and the clavicles display an exaggerated S shape. The patient is very short-coupled, the lower ribs resting almost on the crest of the ilium. The arms are relatively long, and reach almost to the knees.

The heart and lungs are negative; the pulse is soft, and not accelerated. The patient is nervous, but there is no tremor.

There is a marked condition of knock-knees, and the femora are bent outward (Figs. 1 and 2); the legs below the knees are moderately saber-shaped. The

right humerus presents all the usual signs of a fracture at the junction of the lower and middle thirds. (Fig. 3.) One unusual feature, however, was the small amount of subjective pain, and the lack of pain and tenderness on manipulation. Even the rubbing of the fragments did not elicit much discomfort. A radiogram showed the fracture to be transverse.

The patient was seen within an hour after the accident; and it was particularly noted that there was no wrist-drop indicating involvement of the musculospiral nerve.

Lateral wooden splints, well padded, were applied to the arm; and the shoulder and elbow were immobilized by bandaging the arm to the chest, a triangular wedge, base down, having previously been interposed between the arm and chest.

Urinalysis and blood count showed nothing abnormal.

Subsequent history: Thirty-six hours after the accident, a complete musculospiral paralysis had made its appearance. There was a complete wrist-drop, with an area of anesthesia along the radial and lower aspect of the forearm and hand. The muscles of the upper arm were not affected. This paralysis continued unchanged for three months, when there was the first sign of returning motion, although sensation began to improve earlier. In another two months he could extend the hand to the horizontal; and after another month, namely six months in all, he was able, although with effort, to roll cigars, and even to practice on the piano.

The fracture was very slow in uniting. Four months after the accident, the fragments, which were in good apposition, could be bent at the seat of fracture. It was not safe to remove the splints entirely until after the sixth month.

REMARKS

This patient gives a typical history of early rickets of a severe type; and there can be no question that his skeletal deformities and the abnormal fragility of his bones are the result of this disease.

There were eleven fractures in all, of which nine involved the femur and two the humerus. The reason these two bones were especially involved is apparent after consulting the skiagrams, which show the diaphysis of the bones mentioned to be abnormally thin. A slight trauma sufficed to produce a fracture. (Figs. 1, 2, 3 and 4.)

This case illustrates further the importance of the rôle played by the musculospiral nerve in fractures of the shaft of the humerus. It should be an inviolable rule to test the condition of this nerve every time the patient is seen, from beginning to end. Musculospiral paralysis is not uncommon as a complication of fracture, and is of two varieties, namely, primary and secondary. The primary type is so called because it is in evidence immediately after the accident. It is the result usually of a complete division of the nerve by the traumatism incident to the fracture. The secondary type does not become apparent until the lapse of one or more days, and is a result of a contusion of the nerve or a partial division with preservation of continuity. Again, the paralysis sometimes appears late, and is not noticed until four to six weeks have passed. These cases are the result of gradual compression of the nerve by callus-formation, or of pressure-necrosis, the result of the nerve riding over a sharp edge of bone.

Cases of primary paralysis rarely recover spontaneously. Cases of early secondary paralysis generally recover. Cases of late secondary paralysis may, or may not, recover without operation, depending upon the persistence of the cause. The occurrence of pri-

mary paralysis is, in the opinion of the author, a valid indication for operative treatment during the first ten days. If after four to six months a secondary paralysis has shown no signs of recovery, the seat of injury should be explored.

It is obvious of what vast importance it is to make a correct and early diagnosis of the type of paralysis when this complication occurs; for then only is it possible to make a truthful prognosis and conduct the proper treatment.

Case 2.—Acute osteomyelitis of the tibia; necrosis of the lower end; resection; failure of regeneration; bone-transplantation.

J. S., aged 14 years, schoolboy. The patient has had measles and diphtheria in early childhood, but has been perfectly well for years up to the time of the present onset. During the last year he has been growing very rapidly. Several days before the onset of the present trouble he was kicked on the right shin while at play.

Five days ago he was suddenly seized with pains in the lower part of his right leg. The pains became rapidly and steadily worse, and he had a chill followed by high fever. He rapidly became very sick, the pains continued unremitting, and there were frequent chills. This continued for five days, during which time the lower portion of the leg had become swollen and reddened. At this time he was admitted to the Alexian Brothers' Hospital.

Examination: The boy is poorly nourished and tall for his age. He is of septic appearance and semi-delirious. The temperature is 104° F., and the pulse 130. Blood-count showed 4,500,000 reds and 20,000 whites. The urine contained traces of albumin.

The general physical examination revealed nothing abnormal. The lower half of the right leg was greatly swollen, and the skin reddened and shiny. The limb

was excruciatingly tender to the touch, especially at its lower aspect anteriorly, where there was sense of deep fluctuation.

Operation: An operation was immediately performed under ether anesthesia; and a long incision was made anteriorly the length of one-third of the tibia. Free pus was found in the subcutaneous tissue, and upon splitting the periosteum widely it was found that this membrane was separated from the tibia by a collection of pus extending over one-third of the lower end of the diaphysis. The periosteal separation halted at the lower epiphysis, which did not seem to be involved in the suppuration. At the lower end of the diaphysis minute beads of pus exuded from the cortex at several points. The medullary cavity was opened with a chisel, and its greyish-yellow contents revealed, infiltrated with pus. The cavity was wiped out as far as it was involved in suppuration, but was not curetted. The entire wound was then swabbed out with tincture of iodine, and left open throughout, to provide for free drainage. The staphylococcus pyogenes aureus was obtained in almost pure culture.

The condition of the patient improved at once, and all signs of sepsis soon disappeared. On the seventeenth day the wound was filled with healthy granulations, and the discharge of pus was moderate. The lower epiphysis was separated from the diaphysis, and there was free mobility at this point. In the lower angle of the wound the greyish, lustreless bone lay exposed, denuded of its periosteum. It was obvious that the lower end of the diaphysis was necrotic, and that a separation of the dead bone from the living had already occurred below. (Fig. 5.) Where the upper limit would be it was impossible to determine until the dead portion had sequestrated.

There was a choice of two methods of procedure: first, conservative treatment, consisting in waiting

Fig. 1. Left femur, lateral view, showing distortion and unequal thickness. The shaft has been broken three times.

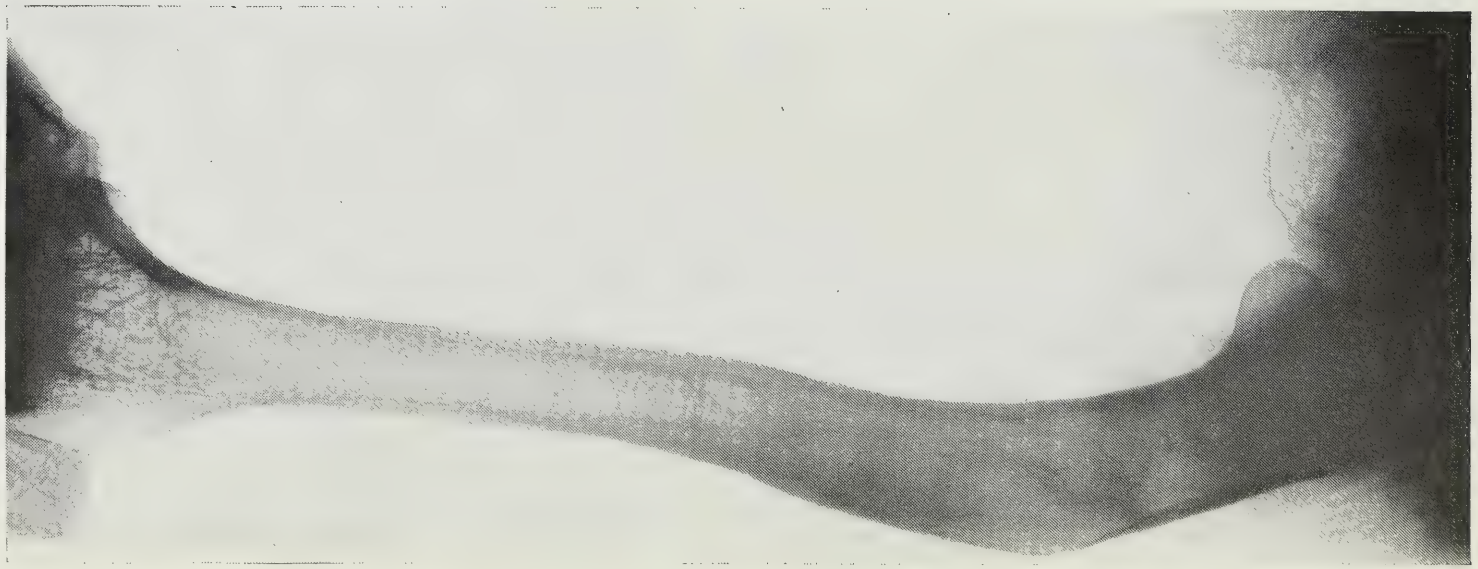


Fig. 2. Lateral view of left femur fractured five times.

Fig. 3. Note the marked curvature of the clavicle. The shaft of humerus is of small diameter compared with the ends. The bone seems to be of less than normal density.

Fig. 4. View of the pelvis, shows old united fracture of the neck of the right femur and of the upper end of the shaft on the other side.

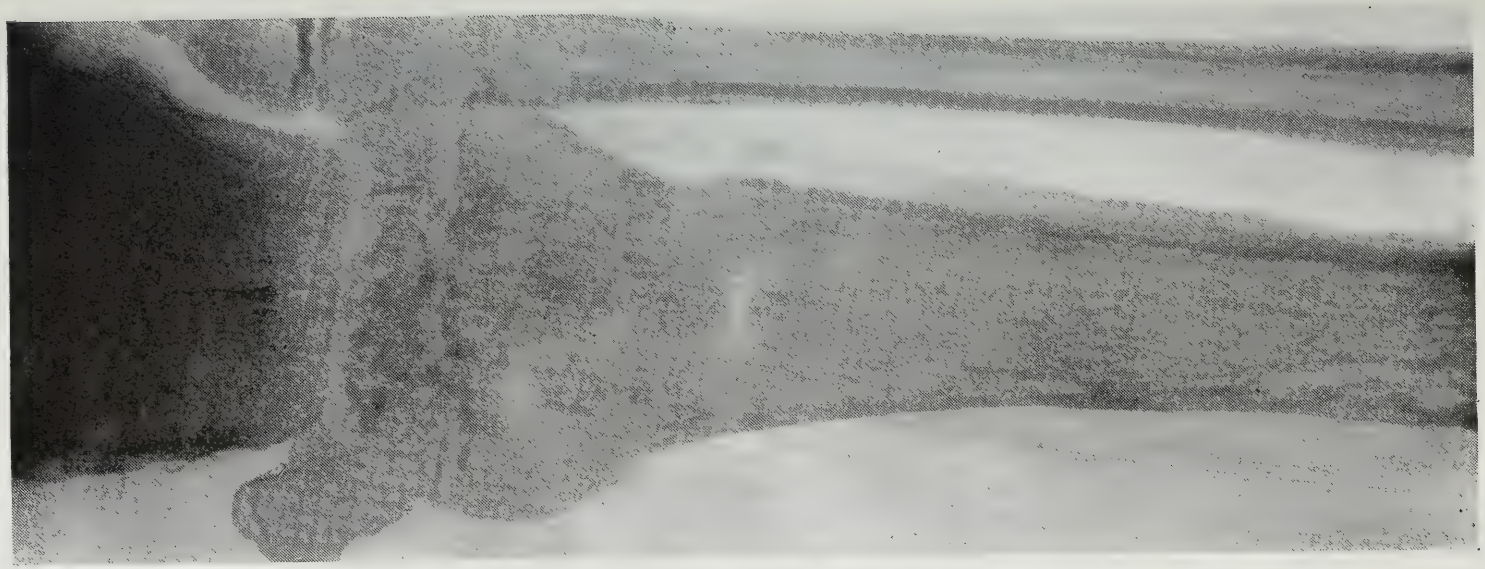


Fig. 5. Acute osteomyelitis of the tibia, fifteen days after the onset, showing the necrotic shaft being surrounded by a deposit of new bone, forming the involucrum. The separation or looseness at epiphysial line is not in evidence in the picture.

Fig. 6. Specimen from Case 2. Lower portion of the tibia removed seventeen days after the primary operation for acute suppurative osteomyelitis. The lower epiphysis had spontaneously separated. Note the massive deposit of new bone or involucrum. Only the lower portion of the shaft was not followed by the regeneration, due in all probability to the taking away of this new bony formation.

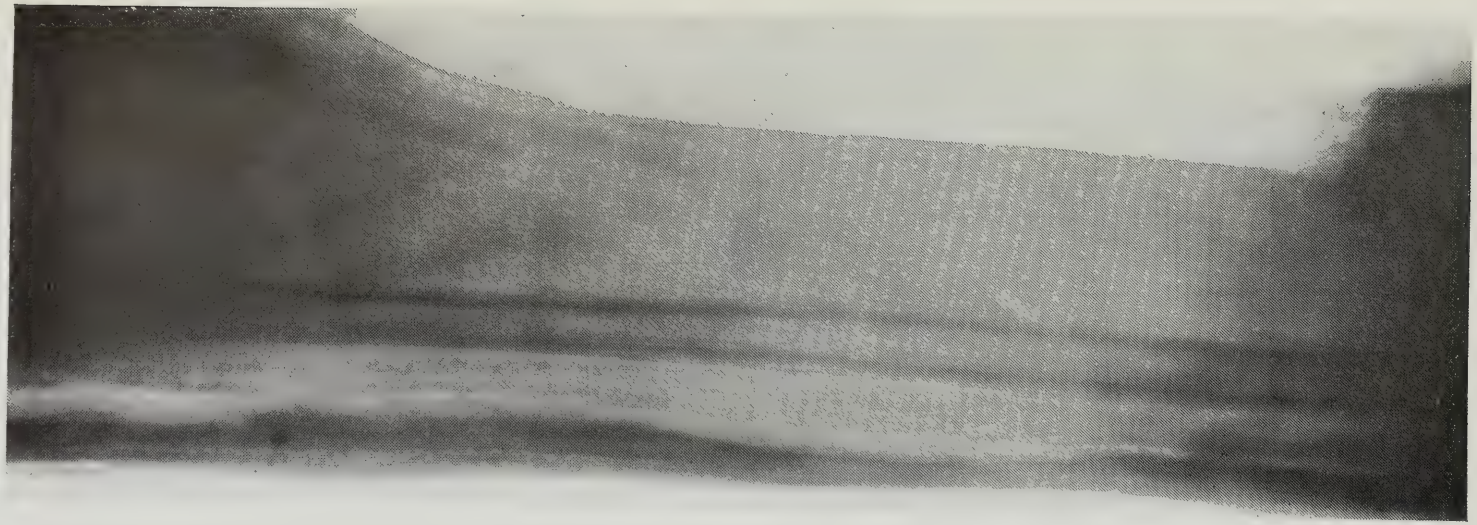


Fig. 7. Radiogram, taken through cast shortly after resection.

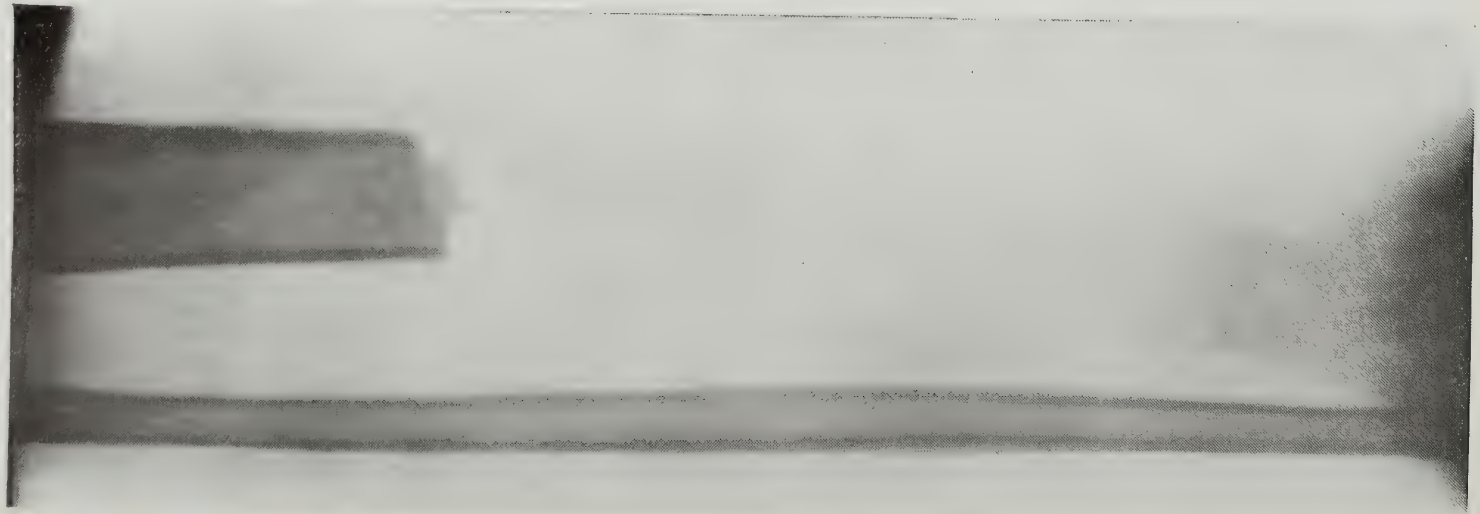


Fig. 8. Radiogram showing the appearance of the bones six weeks after resection.

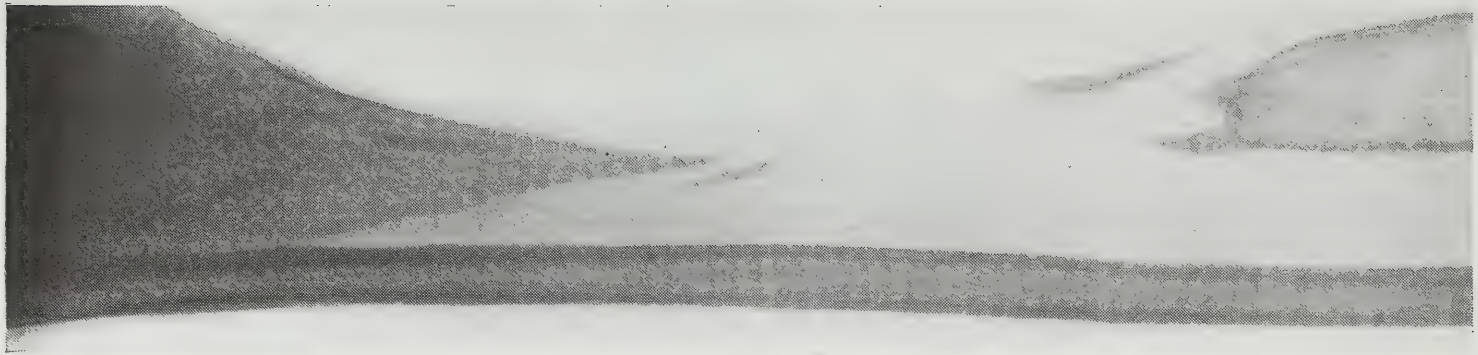


Fig. 9. Eight months after resection. Feeble attempts at regeneration above and below from the osteogenetic elements at the cut ends. No periosteal regeneration.

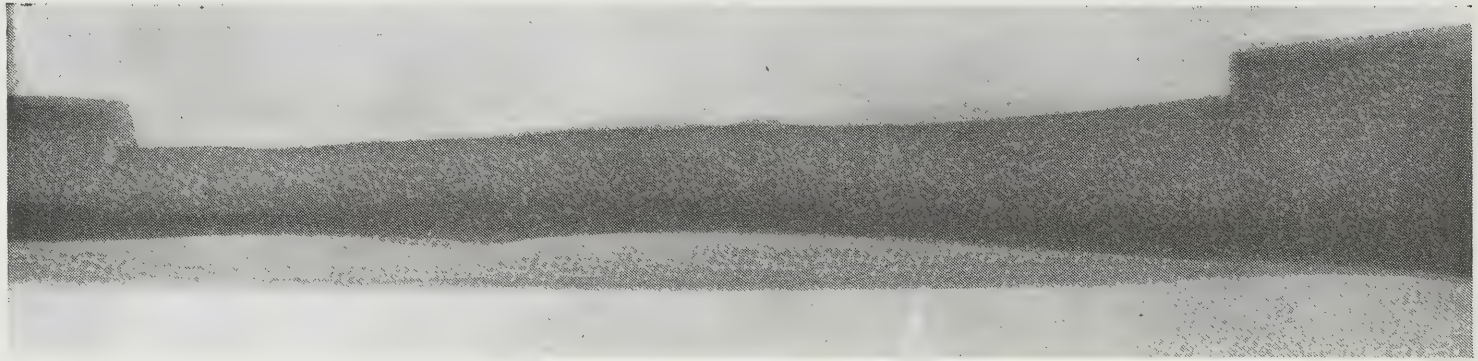


Fig. 10. Skiagram showing tibia from which a transplant measuring eight by three-quarters by one-half inches was removed.

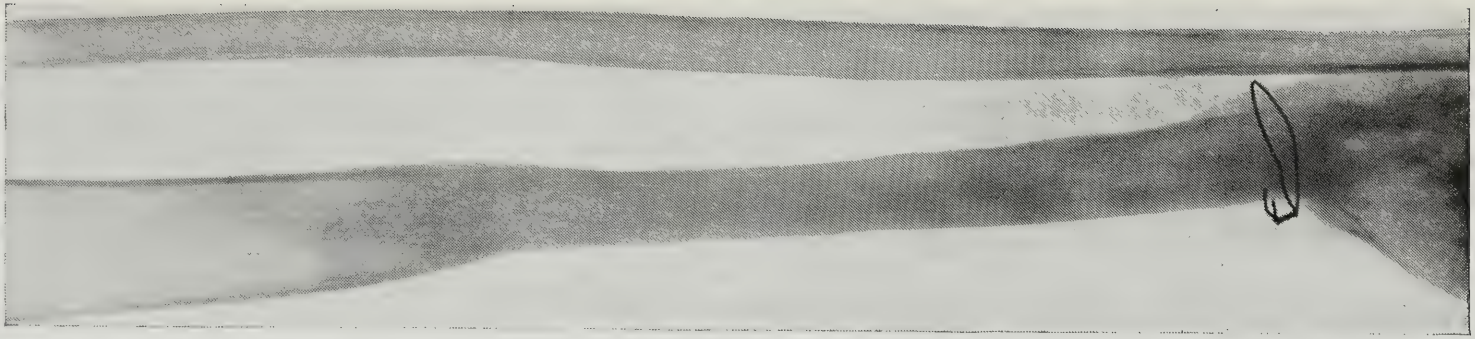


Fig. 11. Shows transplant in place. Taken through cast soon after operation.

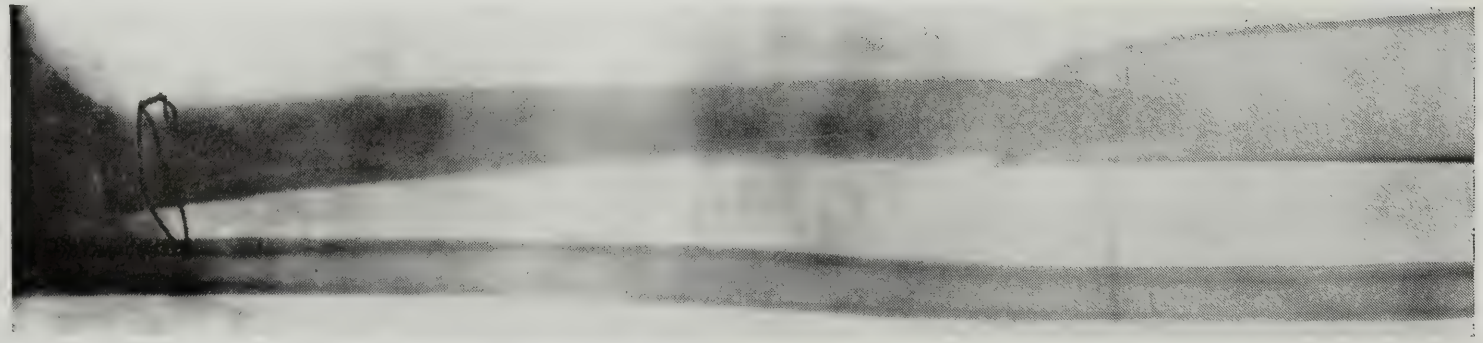


Fig. 12. Skiagram showing transplant one year after operation. The graft has become evenly fused at point of contact, and has increased in size. Patient is walking with perfect freedom.

Fig. 13. Entire diaphysis of tibia removed from a boy ten days after the primary evacuation of pus, the results of acute suppurative osteomyelitis. There had been spontaneous separation of the epiphyses. Note the absence of involucrum deposit. After six months the bone had regenerated completely with a perfect functional result.

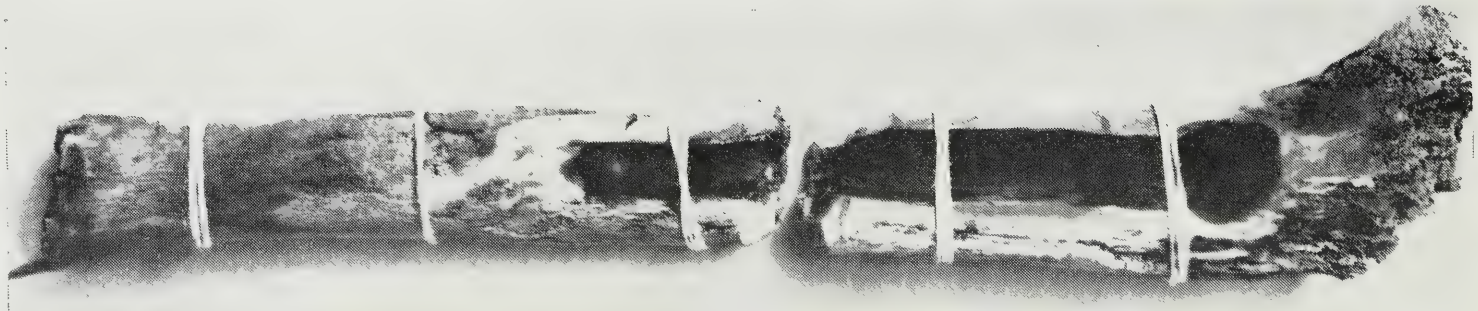


Fig. 14. Entire tibial diaphysis removed from a boy twelve days after the primary operation for acute suppurative osteomyelitis. It was a severe infection, and both epiphyses had separated. Note that there is a slight deposit of involucrum on this shaft. After eight months the shaft had completely regenerated, and the leg was functionally as good as the other one.

until sequestration should have been completed, and then removing loose sequestra, if any; and, second, subperiosteal resection of a portion of the shaft which shows signs of necrosis, and trusting to regeneration to fill the defect.

The latter method was chosen, hoping thereby to shorten the period of convalescence; but in the light of later events the wisdom of this course might justly be questioned.

The wound was enlarged upward, and the lower third of the tibia exposed. The shaft was sawn through at a point where the periosteum had resumed a normal appearance, and the bone was of about normal thickness. The periosteum below was attached rather loosely to the bone; and there was already a deposit of new bone for the formation of the involucrum. This, together with the periosteum, was saved from injury as much as possible, and the loose bone shelled out without difficulty. (Fig. 6) The edges of the periosteum were sutured; and in some places there was so much calcareous material adherent that it was difficult to introduce the needle. The skin was sutured, and a small drain placed at the lower angle of the wound. During the operation the curette was not used, nor were strong antiseptics employed. The specimen removed was seven and one-half inches in length, and showed generous deposits of new bone on its surface, especially at its proximal end. The leg was placed in a fenestrated plaster-of-Paris cast.

Subsequent history: In about four weeks the wound had entirely closed. Radiograms were taken every month, but there was scarcely any regeneration apparent. The leg was kept in a cast for eight months, at which time the appearance was as in Fig. 9.

As the skiagram taken eight months after the resection showed very feeble efforts at osteogenesis, I

felt that it would be hopeless to expect regeneration of bone to fill out the defect. A transplantation was proposed; but the parents demurred at the thought of another operation, and the patient passed from my observation.

After four months the boy was brought back to me for treatment. It was learned that the cast had been removed, and that the boy had been walking around unsupported, bearing considerable weight on the affected leg. It was now one year after the resection and another *x*-ray examination showed no further regeneration. Owing to the weight having been borne entirely by the fibula, there was an upward and outward displacement of this bone, which was shown by the marked prominence of the upper end of the fibula. For the same reason the foot was markedly everted.

Operation: Ether anesthesia and iodine preparation. A graft was removed from the left tibia, measuring eight by three-quarters by one-half inches. It was detached by means of a sharp, thin chisel and a metacarpal saw quickly and with no difficulty. The periosteum covering the transplant was carefully preserved. Then a long incision was made through the old scars of the right leg. The tapering end of the upper fragment was squared off and the medullary cavity reamed out. In the interval between the fragments it was impossible to identify any tissue as periosteum, therefore, a bed for the transplant was made between the muscles. The projecting end of the lower fragment was also squared, and a right-angled ledge cut in to receive the graft, and secure both an end and a side contact with osteogenetic bone. After the upper end of the graft had been somewhat tapered, the same was driven into the medullary cavity above, where it held firmly without any further fixation. Below, the graft was fitted into its ledge, some traction on the leg being necessary to secure apposition, for the graft had

purposely been made long to overcome the shortening that had taken place. The lower end of the graft was held in place by means of a fine wire. The muscles were sutured over the transplant with a few fine catgut sutures, and the skin sutured with continuous silk-worm without drainage. The leg was encased in a plaster mould. (Fig. 11.)

Post-operative history: Both wounds healed by first intention. The leg was supported first by a cast, later by a moulded splint for four months. At this time the leg was firm, and was able to bear some weight in walking. The boy was encouraged to use his leg while the same was supported by the cast, in the belief that function and weight-bearing would tend to stimulate new bony growth. At six months the boy was walking without any support, and with a slight limp only. One year after operation (Fig. 12) function had been completely restored; but there was still a very slight limp, for the eversion of the foot and displacement of the fibula had not been entirely corrected. The skiagram showed that the graft had increased markedly in thickness, and merged almost imperceptibly into its new bony environment.

REMARKS:

How can we account for the failure of regeneration in this case? The answer to this question should serve as a valuable lesson in the treatment of similar cases. The results in this case bear evidence that at the time of resection all of the osteogenetic elements were unwittingly removed. The active cell-elements in the cut ends of the shaft made an attempt at repair, as shown by the tapered appearance of the bones.

During the acute stage of osteomyelitis the periosteum is separated and lifted off from the bone for a variable extent by the formation of pus. It seems that the osteoblasts lining this membrane can withstand the

action of pus for a considerable time, and do not easily lose their vitality. This is proven by clinical results. Now, when the pus is evacuated from under the periosteum, either by incision or spontaneous perforation, this membrane again comes to lie closely in contact with the bone. The active specific bone-elements lining the periosteum, assisted to a variable extent by surviving cells in the cortex, now begin the formation of a shell of new bone surrounding the entire shaft, and firmly deposited upon the same. This is the so-called involucrum, which is Nature's process of supplanting the old shaft which has become necrotic, the result of suppuration. As the involucrum becomes heavier, the necrotic shaft is being removed by absorption. All of the dead bone is rarely absorbed, larger or smaller portions remaining in the form of sequestra. The sequestra are surrounded by a shell of involucrum, which is perforated to form a sinus, through which is discharged the pus and bony debris. The sinus may be the result of an incision or spontaneous rupture. There is an early stage of involucrum formation, following soon after the evacuation of the pus from under the periosteum. During this stage there is a thin incrustation of new bone under the periosteum which has not yet fused with the shaft. At the end of several weeks the new bone (involucrum) has become closely attached to the shaft, and cannot be separated with a chisel.

In Case 2 I waited for seventeen days after primary evacuation of the pus. I was waiting for the lower epiphysis to separate; but in the meantime the involucrum was growing rapidly and becoming firmly attached to the bone. Resection at this late stage removed all of the active bone-elements emanating from the periosteum. Had the resection been done ten days earlier, the thin involucrum with the periosteum could have been preserved in toto, and there would probably

have been regeneration. This would seem to indicate that in cases where a resection is contemplated, one should not wait too long for separation of the epiphysis, but resect early before the formation of involucrum has made much headway.

This plan was followed in several cases in which the entire diaphysis was removed, and was followed by complete regeneration. (Figs. 13 and 14.)

Conservative treatment of acute osteomyelitis consists in the early evacuation of pus from the soft parts and marrow cavity by free incision and trephining, and then waiting for nature's reparative process to eliminate the necrotic bone. Such cases drag on for months or years, and demand frequent operation for the removal of sequestra; and during all this time the patient is afflicted with one or more discharging sinuses. I believe that the period of convalescence will be much shortened, and the local results will be better, in cases that are treated by resection of a part or all of the necrotic shaft, provided that the patient is seen early enough, so that the proper time for resection can be chosen, for then regeneration may be confidently looked for. If the time of election for resection has passed, the cases would better be treated along conservative lines.

DISCUSSION

Dr. A. E. Hertzler (Kansas City, Mo.): I just wanted to repeat briefly what I said at Chicago before the Surgical Section relative to the cause of non-union in the use of metal plates. It can be easily demonstrated that wherever metal is placed in tissue a considerable exudate forms about this metal, which does not coagulate. Now, primary coagulation, formation of fibrin, is absolutely essential in the first steps of the union of any tissue. If the prevention of accumulation of this material is done, healing would take place. That, of course, would demand a preliminary drainage with all the objections that go with it.

Dr. A. F. Jonas (Omaha, Neb.): This paper involves so many points in connection with bone diseases, that it would be absolutely impossible to touch upon all of them, but I would like to take up certain points.

First: In reference to transplants, the transplantation of living bone to infected tissue, and I would like to illustrate what I wish to say on the subject by citing a given case. A young man about twenty-five years of age, a baker, while riding a motorcycle collided with an automobile and sustained a compound fracture at the junction of the lower and middle third of the right tibia and fibula. The fractured bones were placed in apposition by the attendant who was called to see him immediately,—infection followed,—and after a splinting of about four or five weeks there was no attempt at union or clearing up of the infection. The tibia was then wired with silver wire. The wire was removed after two or three months and then a Lane plate was applied which remained in position for about two months. Then he came under my observation. I removed the plate, but there was no attempt at repair of any kind. There was a profuse discharge of pus which caused redness and irritation around the wound, several sinuses were present. We curetted these sinuses a number of times without very much success. The infection continued, and the suppuration process did not subside. So, finally, we made another curettement, a very extensive one, and then transplanted a rib into this infected tissue. We took off the right twelfth rib, split it flatwise in two from end to end and put one of these split ribs on either side of the fracture, pushing them down as far as we could. Union between the ribs and the tibia became firm. The infection gradually subsided, and in the course of two or three months he was able to walk on his leg. He is now working at the baking table every day, and earning his living. The result of this experiment was a very great surprise to me. It succeeded so well that I feel we have a new aid in the treatment of delay and non-union in compound fractures.

With reference to infection after plating of bones, many have said they had non-union of bone after plating. We have had no non-union, but I have seen many reported, and the reason given is that the bones are so fixed that there is no mobility. It seems that some irritation is necessary to bring about the osteogenetic process. So, in our last cases, we have fastened the screw so that the plate is loose on the bone. After fastening the plate firmly, the screws are turned back-

ward about a half-turn. This gives the bone a slight motion, and we think that the callus formation is much more rapid and that it is quite an improvement. So don't let us throw away our Lane plates because there have been so many non-union cases. The tendency is to screw these plates down firmly like a cabinetmaker repairing a chair.

With reference to the last picture thrown on the screen, with the broken Lane plate, we have had the following experience: We had an engineer who had fractured his right thigh at the upper and middle third. Union had taken place at a very bad angle; the deformity was bad. The fragments had become firmly and solidly united. The leg shortened and the function was very much interfered with. The man was not able to return to his work. We decided to refracture the femur and plate it. By the way, the skiagram showed the broken bone overlapping at about that angle (illustrating same) without any new callus. New callus does not throw much of a shadow upon the *x*-ray plate. In spite of this there was a hard, firm callus uniting the fractured bone. In operating, this case, in the chiselling out of the callus I spent about an hour, with a heavy mallet and a strong chisel. We put in our plate with three screws at each end, obtained a very good apposition of the bone. We exercised every possible care in our aseptic procedure, and yet we had a very severe infection, the only infection we have had in our work of this kind. The pus which discharged from that wound was large in quantity, very fetid, very irritating. Temperature was 103° to 104° at times. We opened the wound widely, and the plate was found to be lying loosely against the bone. In the upper fragment there was a necrosis of the shaft of the bone for an inch and a half, it looked as if it involved the entire thickness of the bone. We could not make transplant of the bone, the infection was too intense. We did not like to advise amputation, which for a time seemed unavoidable. After a time, the infection began to subside. We decided to wait, hoping that the bone necrosis would prove to be less extensive than was apparent. We left the plate in position about three months. Then it became very loose and we took it out. We found there was bony union between the inner ends of the fragments, which was strong and firm. He is walking and appears perfectly well today. So I think, in the absence of any serious constitutional symptoms in the way of sepsis, where the seat of the bone plate has become infected, it is well to leave it alone. Provide for ample drainage, open the wound widely and be

sure there is no burrowing of pus away from the seat of operation.

Dr. Geo. M. Gray (Kansas City, Kans.): The paper is very interesting. It brings to my mind one point that is always of interest to me in these cases of acute osteomyelitis, and that is as to how soon it will be best to remove the sequestrum; that is, where they are operated on early. I understand from the doctor's specimens that he exhibited here, that in the two in which the shaft was removed late, there was new formation of bone, but in the one that was removed early there was no attempt at new bone formation. The point that I wish to make is this: How early is it best to remove the sequestrum? To illustrate this, I think you are all familiar with cases that come under your care perhaps two weeks after an infection that loosens the whole periosteum from the shaft of the bone, and the first operation is done for drainage. Then we have a bare shaft of bone, perhaps tibia or femur, which later must necrose. I have at the present time a case in a baby fourteen months old, in which there was an acute osteomyelitis of the femur. It was operated on for the purpose of drainage, free incision being made almost the whole length of the thigh, exposing the whole shaft of the femur, which was denuded of all periosteum. Now, the question arises, When would it be best to make an effort to remove the shaft? Of course, there will be dead bones, probably the whole shaft from the upper to the lower epiphysis. Will it be best to wait until an involucrum has formed?

Dr. C. E. Black (Jacksonville, Ill.): I would like to make mention of a method of plating in which I am interested, a plate which does not use its breaking strength for support. The plating is done with thin steel ribbons varying from $\frac{1}{128}$ inch to $\frac{1}{64}$ inch in thickness and from $\frac{1}{4}$ inch to $\frac{1}{2}$ inch in width. The plates are so placed that the strain is in the direction of a pull upon the screws, instead of a test of the breaking strength of the plate. Another point is the use of screws which do not go beyond the dense bone. I consider this a very important matter, because if you are unfortunate enough to have infection the screws carry it too far. There is no holding strength in a screw that goes beyond the dense bone.

Since reporting a year ago, I have had the privilege of plating a fracture of the femur with these thin ribbon plates, and with perfect satisfaction. The case furnished the opportunity

to lengthen the leg two inches from a shortening which had occurred in a previous fracture. There was the second fracture in the same bone, occurring six weeks after he had left the hospital, with union of the first fracture, from which there was a two-inch shortening. I lengthened out the leg and put on these thin ribbon plates so as to take advantage of the pulling rather than a breaking strength. I got practically perfect result with a full-length bone.

I have not had a great deal of experience in bone-plating. In the cases I have treated in this way I have never had an infection, which I attribute partly to the fact that the plate can be so thoroughly covered and kept far away from the skin, and partly to the fact that I have used these very short screws that have not gone beyond the dense bone.

Dr. Leonard Freeman (Denver, Col.): I want to express my thorough agreement with Dr. Hessert when he says the Lane plates should not be used in the treatment of infected compound fractures. What possible sense is there in putting something into a fracture of this kind which you are perfectly certain will have to come out again? And it is also dangerous to leave a large foreign body in such a fracture. But I wish also to add to this that I do not think a Lane plate should be used in *any* compound fracture, whether there is apparent infection or not. The same reason holds good. We are simply inserting something that may have to come out again, to say nothing of the accompanying danger. The thing to use in infected, or seemingly non-infected, compound fractures is some form of external clamp, which can easily be removed whenever desirable. The drainage is better. Infection is not apt to be so severe as if there were no drainage, as is often true with a Lane plate; and when anything untoward happens, it is easy to remove the external clamp without a special operation and without a general or even a local anesthetic. These external clamps, I am convinced, are not used enough. Surgeons are commonly using the internal plates for perhaps two or three reasons—they think it is their duty to use them, or they wish to show others that they *can* use them, or, perhaps, they are not familiar with the merits of the external clamp.

Dr. James E. Moore (Minneapolis, Minn.): While I share the consensus of opinion that bone plates should not be used in compound fractures, I had the pleasure of hearing a paper read by Dr. Frank Jonas, of Baltimore, day before yesterday in

Atlanta, in which he stated it was his practice to apply the Lane plate in compound fractures; that he had done it in a large number of cases, and got better results than in any other way. He maintained that taking out a plate was a trivial matter, had to leave the wound open anyway, and did not complicate matters when he had infection. Dr. Jonas' experiences, just related, would rather bear out that opinion. So it seems to me that we may be able to use the plate in some of these compound fractures in which we have wanted so badly to use it, and have very fair results.

Dr. William T. Reynolds (Kansas City, Mo.): Those who recall Mr. Lane's technic of applying the plate will not fail to remember the stress he bears upon the importance of rigid asepsis. Closed fractures of the long bones which cannot be placed in apposition and maintained should have a plate to hold them in place; of this there is no doubt. Compound fractures have no surgical interest as far as the plate is concerned unless the wound in the soft tissues is left wide open.

I want to emphasize what Dr. Jonas has said about putting in the screws, a plan I adopted eighteen months ago. Subsequent skiagrams prove the correctness of this theory. The pictures show that the screws have scarcely moved, though put in loosely.

Dr. Willard Bartlett (St. Louis, Mo.): I have studied this subject for the last two or three years, since visiting Mr. Lane's clinic. The results in these cases convince me that there is one factor more than any other involved in the non-union of fractures, other than mechanical factors in the manipulation. I would say this, that during the last eighteen months we have made in our office a Wassermann in every case of non-union and have had a positive result in every instance. Some of these non-unions, one followed plating and bone-grafting secondarily in our own hands, with no result until the patient was placed on anti-syphilitic treatment until a firm union was obtained. Of course, I do not mean to state that syphilis is the only cause of non-union, but I want to say that we can no longer dismiss the subject without making the Wassermann test.

Dr. Clarence M. Nicholson (St. Louis, Mo.): I am rather convinced from my experience that the length of the screw, the fact of its being screwed in tightly, or the thickness of the plate, has little to do with the ultimate result. After watching Lane for six weeks, I asked him how many plates he had removed. He replied he had never taken out a plate, though he had not plated compound fractures. I am therefore led to believe that the question of non-union, not infrequently, is a question of sepsis. If there is absolute asepsis, I believe the failures are few and far between. Any man who puts on a plate, allowing his hand or an instrument which has been handled even by a gloved finger to enter the wound, introduces a possible measure of infection.

Dr. Malcolm L. Harris (Chicago, Ill.): I just want to say a word relative to the plating of compound fractures. I favor plating compound fractures in suitable cases even when they are infected. When it is difficult to hold the bones in place in compound, comminuted fractures, a plate put in facilitates the dressing, holds the bones in apposition while they are healing, and very much hastens union; but these cases should invariably be kept perfectly wide open, never attempt to close them. To put in a plate in an infected wound and attempt to close will be followed by disaster. A properly applied plate will hold the fragments in apposition until union can take place sufficiently to permit the plate to be dispensed with, and in such cases the plate will be an advantage, but the secret of success lies in keeping the wound wide open and drained and in removing the plate at the proper time.

Dr. D. S. Fairchild (Clinton, Iowa): When we first began to use the plates, about fifteen years ago, we used them almost exclusively in compound fractures. We had to deal with a certain class of cases where the bone was broken into a number of fragments. These fragments were not entirely separated, but could not be kept easily in line because the fracture had been inflicted by direct violence, in railroad injuries, particularly the coupling of cars. We had silver wire plates made, say a yard or half a yard long, cut off pieces as we wanted them and fastened them on bones with screws, particularly in fractures of the tibia. We got very excellent results. We did not know at that time it was a bad thing to do, and we have been more timid about it since we have found it was wrong than before, but occasionally still do it in

cases of compound fracture. As Dr. Harris has said, the wound was thoroughly drained, because in some of the cases we could see half the length of the plate after we had brought the tissues over it.

Dr. Hessert (closing): I did not wish to create the impression that I would dogmatically say that no compound fracture should ever be plated. Dr. Harris has said that these compound fractures should be very thoroughly drained. Now, if we have a compound fracture with a large external wound or wound we have made large, and a fracture of that kind is very difficult to dress, I have myself put on a Lane plate, left the wound wide open, taken out the plate in, perhaps, a month, and it certainly facilitated union; but as a general proposition I would say that putting in a foreign body in the presence of compound fracture or infection is not as a rule a good thing.

PAROSTEAL CALLUS: THE SO-CALLED MYOSITIS OSSIFICANS TRAUMATICA

(*A preliminary report*)

OLIVER J. FAY, M. D.

DES MOINES, IOWA

Copping[1], writing of intramuscular ossification, in 1740, was perhaps the first to call attention to this interesting phenomenon. In the succeeding one hundred and fifty years occasional papers were written on the subject; and towards the close of the nineteenth century the popularization of the *x*-ray, and the added importance given to all traumatic lesions by the enactment of workingmen's compensation-laws, aroused a new interest in the subject, resulting in a total of more than 500 reports of muscle ossification following trauma.

My own interest in the subject was awakened by a typical case of ossification of the vastus, and was heightened when the next few months brought me 5 additional cases of parosteal callus. In the nature of the injury, in the symptomatology, and in the radiographic revelations, these cases were very typical of the type of muscle ossification here under discussion and for that reason seem worthy of a brief review. In none of these cases did the family history or the personal history up to the time of injury contain anything pertinent to the case; and I shall therefore speak only of the history of the muscle lesion.

In the first of these cases, referred by Dr. T. A. Burcham, of Des Moines, Iowa, the patient, a muscular, well-developed man of 23, had been struck on the thigh during a football game. The leg became swollen and numb, but was not painful. As the swelling subsided, an induration was noticed; massage and deep pressure were painful, and the leg could not be flexed. A hard mass was palpable deep in the quadriceps extensor femoris. The radiograph, which, unfortunately, does not show its entire length, disclosed a long, spindle-shaped shadow, forked at one end, lying parallel to the long axis of the bone from which it was separated by a zone of light. (Fig. 1.)

At operation, five weeks after the accident, a bony mass, some six and a half inches long and two broad, was found lying in the vastus medius, from which it was removed by sharp dissection. After loosening it from the femur, to which it was adherent over a small area, the latter was curetted.

The patient made a rapid recovery. When discharged from the hospital, ten days after operation, the leg was somewhat swollen, and motion in the knee was limited, although walking was not interfered with. A month after operation an *x*-ray was taken, and a light shadow was seen about the point where the chisel had denuded the bone. (Fig. 2.) From then on, radiographs were made at frequent intervals; and while the shadow did not increase in size, it became denser and its outlines clearer. The patient, eager to take up athletics, importuned a second operation; and four months after the primary operation the second mass was removed. It had the volume of an egg, and was encapsulated, adhering to the muscle at one point only, but firmly attached to the bone. Recovery was again very rapid. The knee gradually regained normal motion, and six months later the man was playing the star rôle on his football team.

Both of the specimens were sent to Dr. E. R. Le Count, of Chicago, for examination. He reported that they were practically identical in structure, except that the primary mass showed the presence of an old hemorrhage. Considerable cartilage was present; some of the trabeculæ were very wide; and remnants of the cartilage in which the bone had grown were present, even in the most compact portions. No evidence of the production of red corpuscles was found. The relative absence of osteoid tissue, and the aimless irregularity of the arrangement of the various components, are the chief differences which distinguish this from normal cancellous bone, which unites fractures.

The second case was referred by Dr. L. D. Hewes, of Rockwell City, Iowa. His patient, a woman aged 50, had received a posterior dislocation of the elbow six months before. It had been reduced almost immediately, and treated in the usual manner, but both extension and flexion had remained limited. On examination, a rather long, slender mass, apparently bone, was found in the plica cubiti, pressure upon which raised the forearm. A radiograph showed that both the bones of the forearm and the humerus had clearly-cut outlines while a slender forked shadow, with a light attachment to the ulna, represented the normal position of the brachialis anticus,—a typical picture of the most common form of muscle ossification following a dislocation of the elbow. Some months later the patient returned. Extension was now about 130° and flexion 80° (Fig. 7); and she complained of some pain and swelling of the forearm and hand. The physical and *x*-ray examinations showed no changes. Operation was refused.

Dr. A. R. Hackett, physical director at the Iowa State Agricultural College, sent the third patient to me; and his case is of especial interest because the pre-

vious history suggests a possible predisposition. The patient is an athlete, aged 25; and all of his injuries were received on the football field. Seven years ago he was struck over the insertion of the right deltoid; the arm was very sore for a month, then gradually improved, but a hard knot has remained. There is no interference with motion; and the arm is not painful. There is a hard mass on the palmar surface of the right middle finger, the result of a bruise, and a similar mass in the left hand at the base of the fifth metacarpal, also the result of a bruise received some eighteen months ago. About the same time he was struck over the insertion of the left deltoid. Soreness persisted for several weeks, and a hard knot remains; there is no disability.

Radiographs of the right arm and the right and the left hand, are entirely negative; but a radiograph of the left arm shows a shadow just external to the insertion of the deltoid, apparently representing an ossification of the external attachment of the brachialis anticus. Its boundaries are hazy as if there were no sharp line between ossification and muscle nor between parosteal mass and the bone shaft. (Fig 8.)

The fourth patient, a blacksmith, aged 25, with powerful musculature, was referred by Dr. Anspach, of Colfax. In an attempt to stop a runaway team two months before, he had thrown his entire weight upon his flexed left leg, and had felt something give way in the muscles of the left thigh. The thigh was swollen and painful for a few days. These symptoms gradually abated; but soreness persisted, and the patient had been unable to flex the leg. Three weeks after the accident the attending physician noticed an induration in the muscles of the anterior thigh. This had grown more marked, but had not increased in size. On examination, a mass some five inches long and three broad was palpated in the deep muscles of the

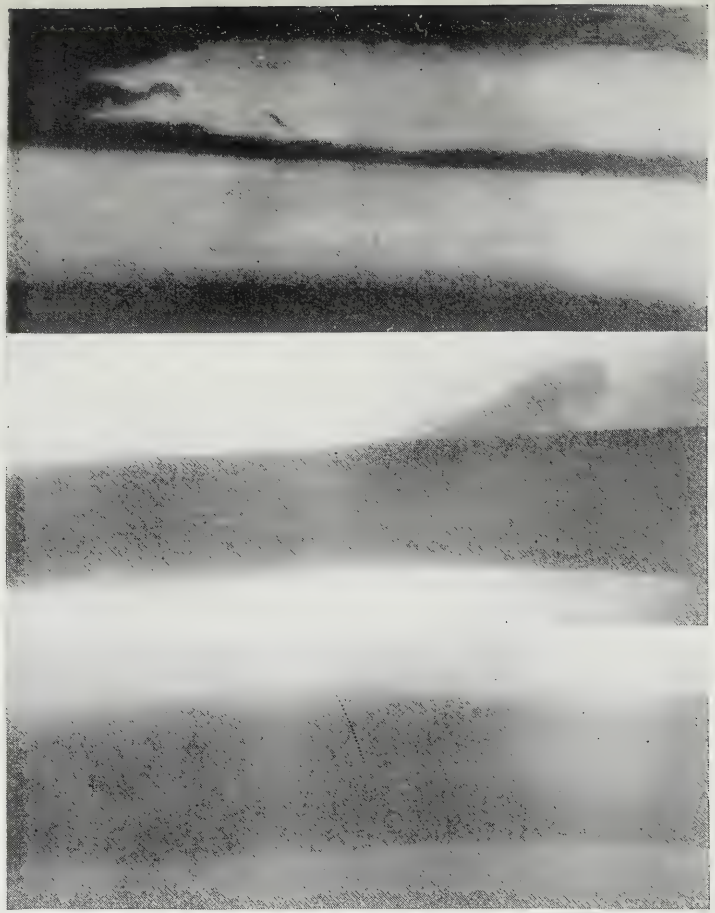


Fig. 1. Case 1 before operation (the entire length of the mass is not shown). The zone of light between callus formation and bone shaft is well defined.

Fig. 2. Case 1 three months after operation. The shadow has assumed definite outlines.

Fig. 3. Case 1 eight months after operation. There is a well-developed "bone scar" but no impairment of function.

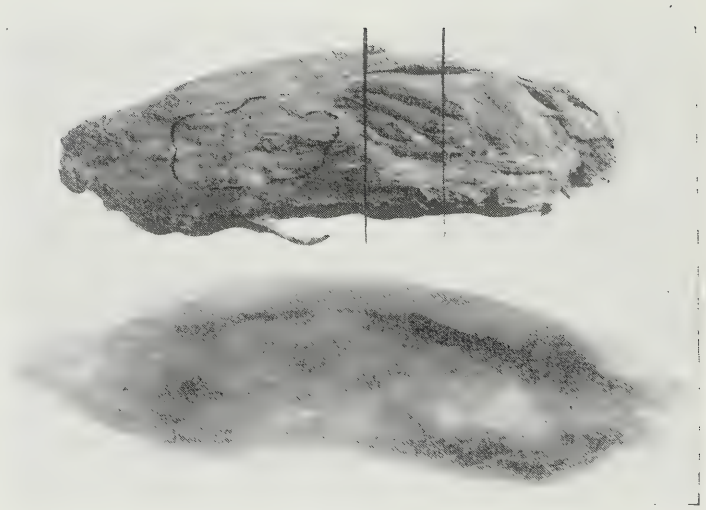
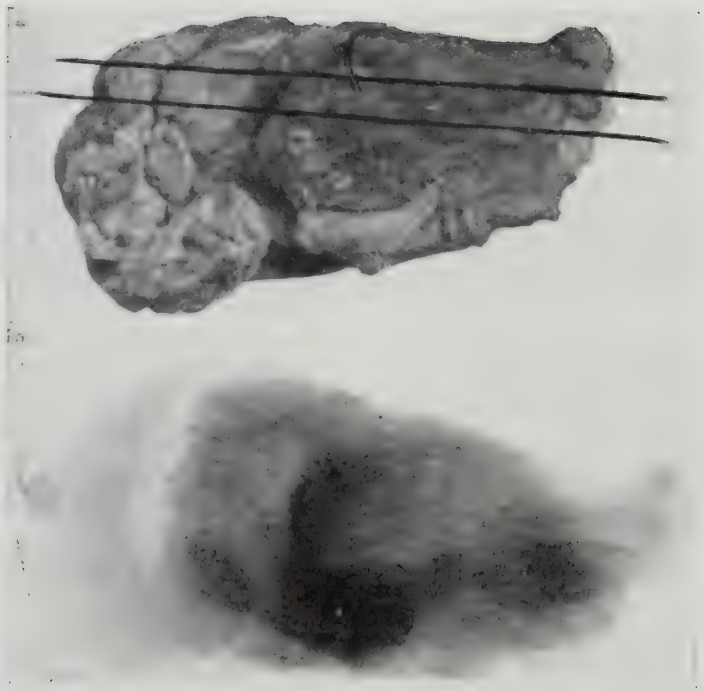


Fig. 4. Reproduced photograph and radiograph of the mass removed (Case 1) at the first operation. Reduced to two-fifths. The irregular area indicated by "a" is where on the side opposite to that shown here this mass has bone on its exterior, a surface made by sawing it away from the femur. The parallel lines are where material was removed for histological examination.



Fgi. 5. Mass removed at the second operation (Case 1) with radiograph. The irregular region indicated by "a" is a surface made by chiseling this mass away from the femur. The parallel lines indicate the piece removed for micriscopic examination.

Fig. 6. Photomicrographic reproduction of a single section. The section is 2 cm. long, 7.5 mm. wide at the narrow, 12 mm. wide at the opposite end. Here the zones are well shown. Starting with the old blood clot (a) as a center, there are (b) places where hemorrhage is still present in the zone of granulation tissue (c); outside of these zones a zone of bone (d) sharply demarked on the one edge but uniting very irregularly with the next outermost zone of cartilage (e); (f) osteoid tissue; (g) spongy bone; (h) skeletal muscle.

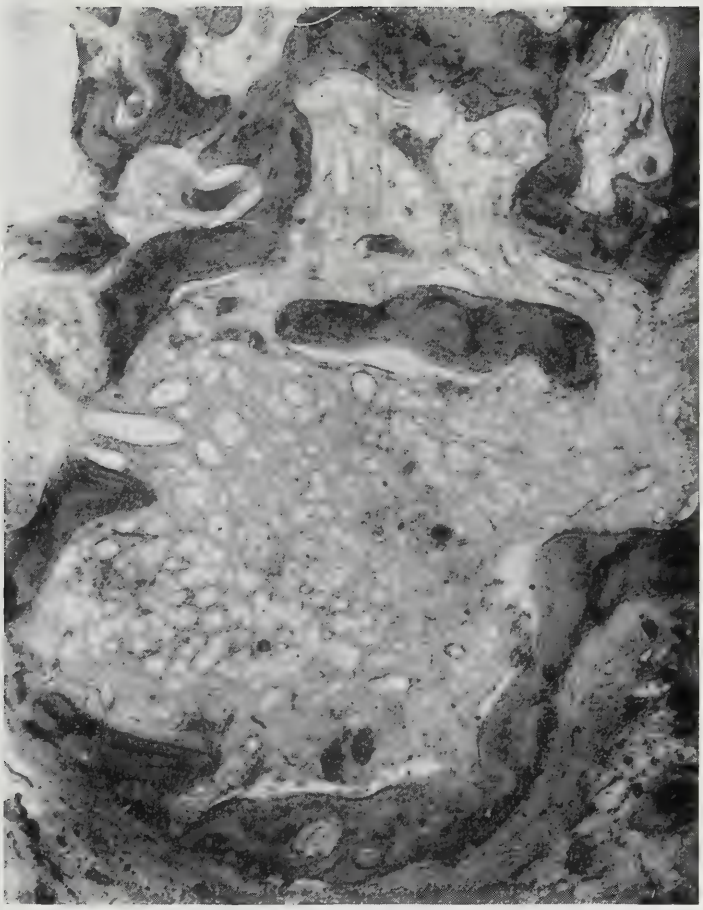


Fig. 7. An unusually large marrow cavity with many places (round holes) where fat formerly was present. Here bone formation is relatively quiescent although nearly all the trabeculæ contain cartilage. 30 Dia.

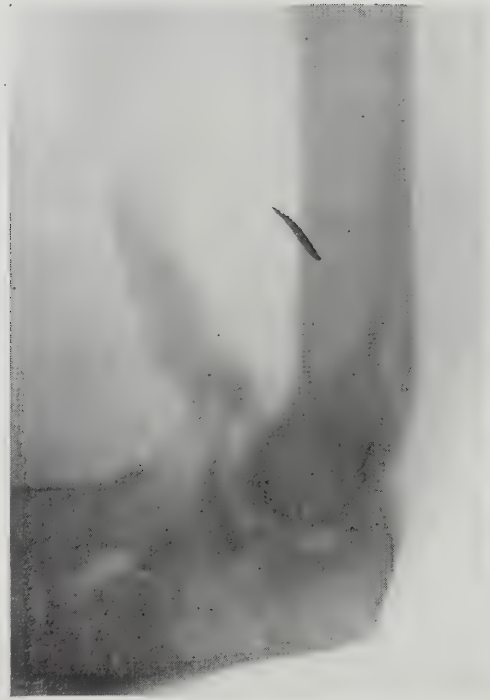


Fig. 8. Case 2 six months after reduction of the posterior dislocation.

Fig. 9. Case 2 six months later showing the extent of flexion.

Fig. 10. Case 2. Limited extension.

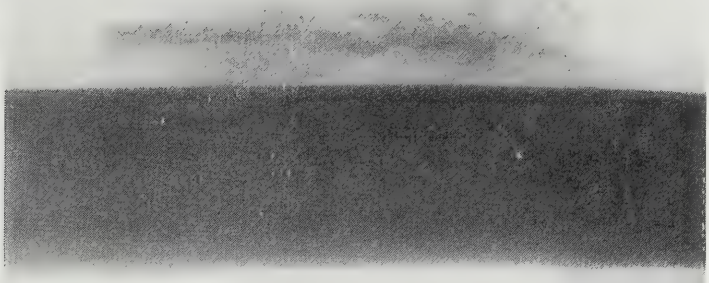


Fig. 11. Case 4. The saddling of the femur by the callus formation prevents an unobstructed view of their relation to each other and, consequently, the zone of light between them is not clearly defined. The "cloud of smoke" appearance is typical.

Figs. 12 and 13. Photograph and radiograph of tissue removed from the fourth patient. Segments taken for microscopic examination indicated by the parallel lines.

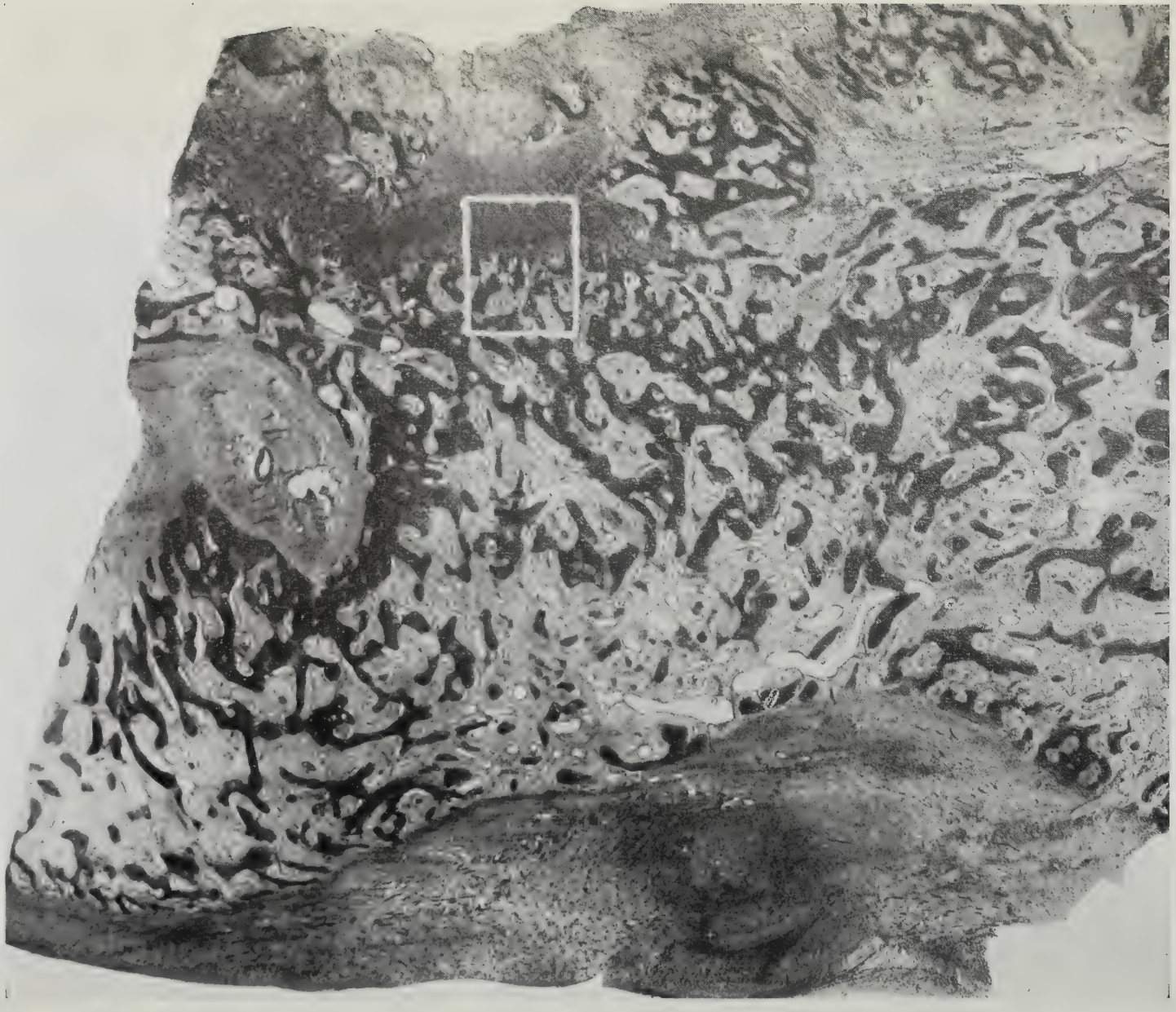


Fig. 14 Bone with an old hemorrhage in the adjacent tissue (below). Above where the square is drawn there is hyaline cartilage This field enlarged is shown in Fig. 16.

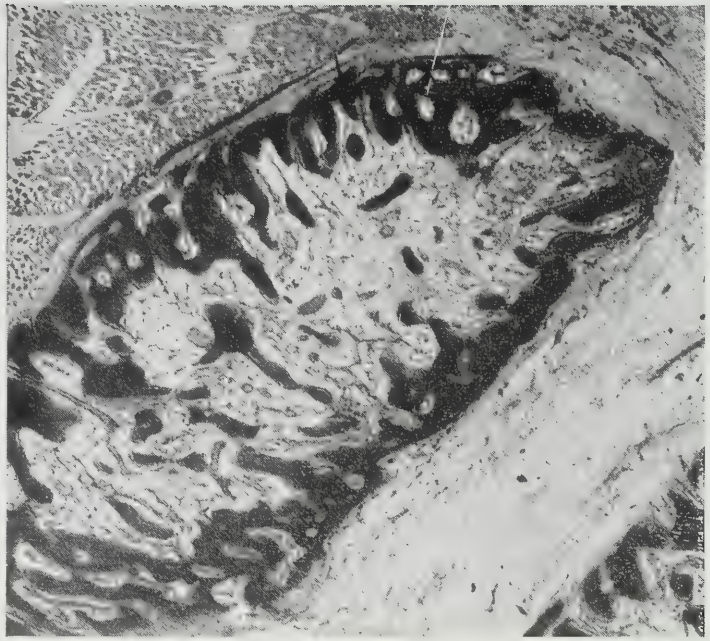


Fig. 15. Small island of bone, illustrating the heavier bony trabeculæ about the periphery. Muscle on one side, fibrous wall on the other.

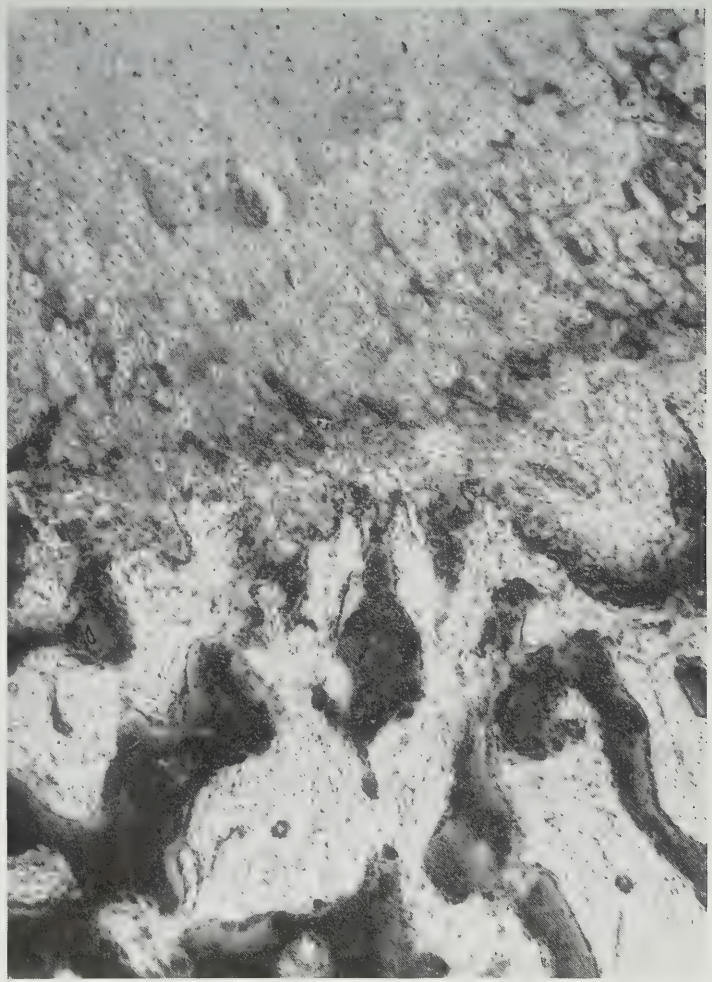


Fig. 16. Reproduction of the junction of cartilage and bone marked in Fig. 14 with appearances very like an epiphyseal line.

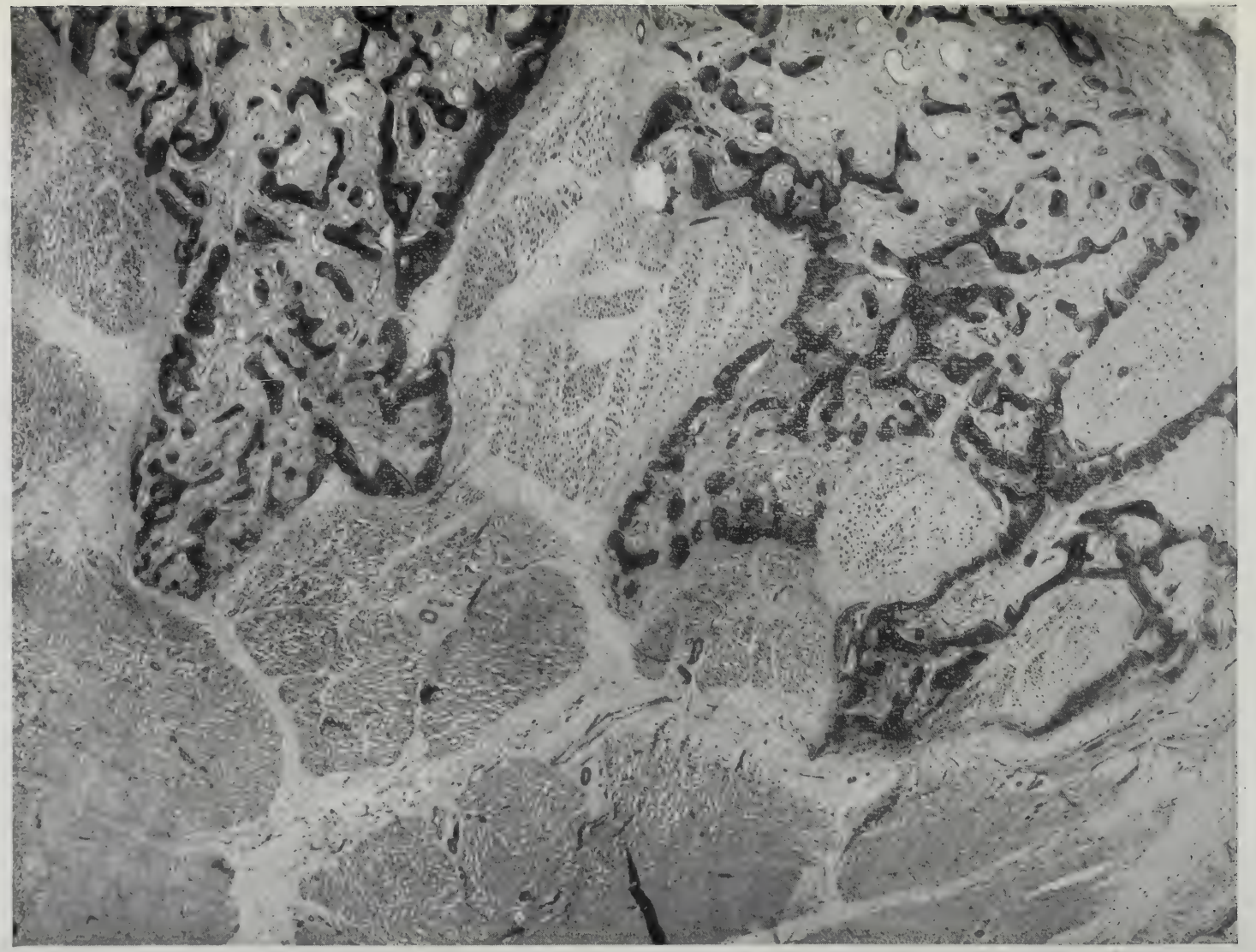


Fig. 17. Showing the intimate relation of muscle and bone.

Fig. 18. Case 5. Anterior dislocation of the elbow seven weeks after receipt of injury. The shadow in the cubital fossa represents a callus formation in the border of the brachialis anticus.

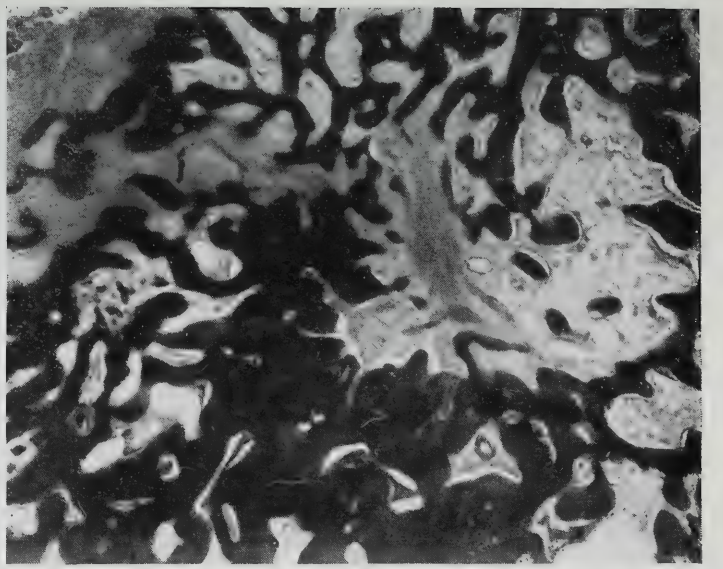


Fig. 19. Very heavy bony trabeculæ and small marrow cavities.

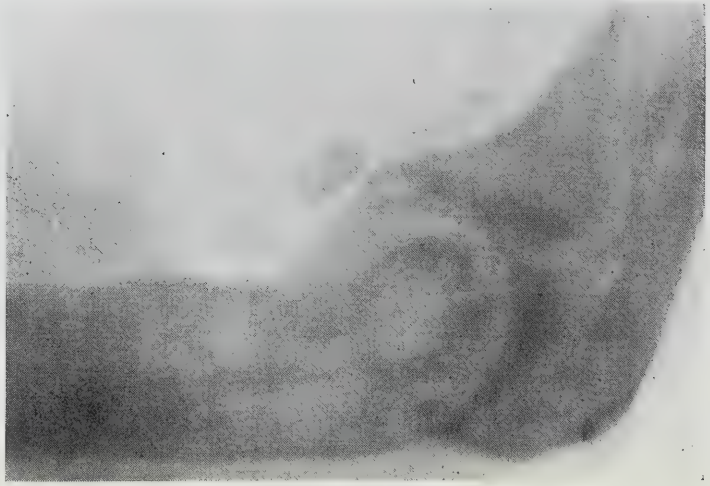


Fig. 20. Case 6. A parosteal callus in the brachialis anticus twenty-two years after the receipt of the determining injury.

middle third of the thigh; it was only slightly movable. The *x*-ray revealed a shadow lying above the femur, like a cloud of smoke. At operation a mass of bone four inches long and some two and one-half inches broad was found in the vastus medius. It was loosened from the muscles by blunt dissection, and from the bone, to which it was adherent, only at its inferior pole, by a prying motion. Its inner surface was concave, fitting the surface of the femur; and this saddling accounts for the absence of a zone of light between bone shaft and callus as seen in the first case. (Fig. 9.) Recovery was uneventful and the functional result excellent.

The next patient, a well-developed lad of sixteen, had suffered an upward and forward dislocation of the elbow seven weeks before. There was considerable deformity and motion was very limited. The radiograph, in addition to the displacement of the bones, disclosed a faint shadow in the plica cubiti (Fig. 10). During the open operation for the reduction of the luxation, a mass of bone one and a half inches long was removed from the border of the brachialis anticus, into which it extended. There was also a slight attachment to the ulna. Recovery has been uneventful; and the range of motion is now some six inches.

This specimen is now being examined by Dr. E. R. LeCount.

The sixth patient, a farmer, aged 30, stated that he had been kicked on the elbow by a calf twenty-two years ago. The arm was painful and swollen for a time, but gradually returned to normal. Since then, however, on any unusual exertion of the arm, the elbow becomes painful and tender. Thirty-six hours before he consulted me, he had helped put chains on an automobile, and had not been able to sleep since because of the pain in the joint and forearm. Passive motion was painful; and a small bony mass could be

palpated in the plica cubiti. On the radiograph a small round shadow was seen here, apparently near the attachment of the brachialis anticus to the ulna. (Fig. 11.) This patient is to return for operation.

The pathogenesis has been, and still is, an open question. The hemic theory, which sought to explain the bone-formation by the organization of the blood-clot, and its transformation into cartilage and then into bone, is, histologically, so heretical that it merits no serious consideration. Bard's theory of aberrant sesamoid bones, maintaining that the callus is due to traumatic stimulation and proliferation of such bones, is scarcely more acceptable. A periosteal origin, from the detachment and inter-muscular transposition of a periosteal flap, or from the migration of bone-cells through a defect in the outer layer of the periosteum, and their subsequent proliferation within the muscles, has had many supporters, and many opponents, the latter having quite generally supported the theory of ossifying myositis. The former have urged, in support of the periosteal theory, the possibility of producing bone by the transplantation of periosteal flaps (Berthier[3]) and by the injection of periosteal emulsion (Nakahara and Dilger[4]); the frequent attachment of the bone mass to the skeleton; and the similarity of structure to that of the callus of fractures. On the other hand, it has been pointed out that the bone produced by periosteal implantation or injection never far exceeds the dimensions of the periosteum from which it has grown; that this experimental work in no way reproduces the conditions of trauma or demonstrates the possibility of traumatic periosteal detachment or dissemination; and, finally, the development of traumatic bone masses far from the bone having been reported, in the recti by Röpke[5], Strassberg[6], Chapelle[7], Sabijiakina[8], De Graene[9], Hannes[10], Lecéne[11], Rubesch[12]; in the glutei by Tubenthal[13] and

Schwartz[14], it is urged that any theory advanced to explain the formation of adherent parosteal callus masses should also answer for the non-adherent which outnumber the former three to two (Cahier[15]). To assume that the bone formation is an inflammatory product of hematogenous infection as does Berndt[16] seems far-fetched. If these callus masses are to be considered of inflammatory origin, we must accept the term inflammation in its broader sense, as a non-infectious process, the reparative reaction of injured tissues. This conception of a regenerative process accords well with the clinical picture,—an injury followed by the usual symptoms of traumatic reaction, the formation of the mass by "organization," rather than by proliferation, as shown by the early attainment of its ultimate size, and the limited period of growth, which sharply distinguishes it from tumor growth. A changed nutrition, a changed relationship to the nerves, and a lack of functional activity, result in a new environment for the cell, which, for the time being, loses its specific function, and becomes morphologically an osteoblast (Borst[17]). Orth[18] recognizes a tissue- and a cell-metaplasia. In both the bone formed is true bone, containing bone-cells and corpuscles, but in tissue-metaplasia the connective tissue ossifies as a whole; in cell-metaplasia the connective-tissue cell loses its differential function and becomes osteogenetic. With the return to normal of the cell-environment, the cell may regain its power of differentiation; but the bone that has formed, since it is no more readily absorbed than other compact bone, is left within the free musculature as an "osteoma," or adherent to the skeleton as an "exostosis."

With this conception of traumatic intramuscular ossification as a reparative process, the term myositis ossificans traumatica seems an unfortunate one, because it suggests an inflammatory, and not a regen-

erative, change. For this reason I have preferred the appellation *parosteal callus.* The traumatic origin and the structure of the mass alike suggest a callus, while the modifying *parosteal* (from the Greek *para* + *osteon,* bone) distinguishes it from the callus of fractures.

The symptoms immediately following the injury which determines a parosteal callus, are those which accompany any contusion of like severity: swelling, pain or numbness, and more or less loss of function. Sometimes a fluctuating mass indicates the presence of a hematoma, but the mass is more frequently of doughy consistency, due to infiltration of the muscles and subfascial extravasation of blood. The subsidence of the pain and swelling is not followed by a corresponding subjective improvement; and the doughy area is found to be indurated, either as a whole or over circumscribed areas. The functional disability increases, instead of decreasing, and the induration gradually becomes osseous.

The history of trauma and the clinical picture may suggest the presence of parosteal callus, but the radiograph is essential to differential diagnosis. Both osteosarcoma and parosteal callus may have a history of trauma; and no symptom of either may be said to be pathognomonic. But in parosteal callus the bone-shaft has clean outlines, its density is unchanged, and, unless the callus saddles the bone interfering with an unobstructed view of its relation to the latter, a characteristic zone of light is seen between the callus and the bone-shaft. A positive diagnosis between osteosarcoma and parosteal callus is often supposed to be impossible. I can only say that I have not yet seen an osteosarcoma which fulfilled these three requirements. In bone-formation in the brachialis anticus (the sequence according to Machol[19] of every reduced posterior dislocation of the elbow), the new-

formed bone is often club-shaped or pronged (Kienböck[20]), and on clinical evidence alone might be considered a bone-splinter. Here again the clear outlines of the bone-shaft, the position of the shadow in the plica cubiti at the point normally occupied by the brachialis anticus, and the shape of the shadow, should make differentiation easy.

Osseous and cartilaginous exostoses are often multiple; and in both the radiograph shows the periosteum pushed up, merging with them. Other skeletal deformities are often present. True exostoses occur at the epiphyseal line, but they may travel upwards with the growth of the bone (Fig. 12), and are most frequently found at points where the skeleton is not well protected by muscles—parosteal callus is most frequent where the musculature is deep. Cartilaginous exostoses often occur on the hands or feet (Fig. 13), while traumatic callus formations are almost never found here. The one or two reported cases were of callus formations on the palmar surface.

The shadow of a parosteal callus in the arm or thigh is usually spindle-shaped, much longer than it is broad, with its long axis parallel to the long axis of the bone; less frequently it is round. In parosteal callus of the brachialis anticus the shadow is usually triangular or club-shaped, the apex of the triangle or the handle of the club often showing some attachment to the coracoid process; where the insertion is not ossified, the shadow lying in the plica cubiti is frequently round.

The radiograph in my third case throws some doubt upon the diagnosis of parosteal callus. The spur-shape, the merging of the bone-mass and diaphysis suggest an exostosis. But the clinical history is that of parosteal callus, and the age of the process would account for the absence of the zone of light, the merging of the connective-tissue fibers of the muscle at-

tachment with those of the periosteum making such a fusion probable in the later stages. (La Pointe[21].)

Preventive therapy and the indications for operation have alike been much discussed. While the number of reported cases is probably no index to the number of those diagnosed, and certainly gives no idea of the actual frequency of parosteal callus, it is true that only a very small percentage of muscle-injuries are followed by callus development. In general, then, it may be said that the best treatment of the primary injury is the chief consideration, but the greatest possible gentleness in the reduction of any dislocation cannot be too strongly urged. When it is remembered that the development of parosteal callus is always self-limited, and that its presence does not in any way endanger the life or health of the patient, the indications for operation narrow down to the question of functional disability. Where the presence of the parosteal callus seriously interferes with the normal occupations of the patient (the question is in part an economic one), or where a nerve or blood-vessel is involved in the mass, operation is indicated. If absolute asepsis is recognized as the first principle of surgery, the danger of an operation is practically only that of the anesthetic. When the callus lies free in the musculature, we may promise much, for, at worst, a fibrous cicatrice is of far greater service than is osseous tissue; and with an adherent parosteal callus the outlook is scarcely less bright. Where peri-articular ossification is present the prognosis is doubtful.

The danger of recurrence, as I see it, brings us to a discussion of two other points: time for operation and operative technic. If operation is undertaken before the damaged tissues have regained their differential function, any of this tissue left behind at operation may lead to the formation of a second callus. An early operation must therefore be radical, but better

functional results are obtained by waiting until the bone-mass has "ripened," as shown by the correspondence of the radiographic image with the palpable mass, when as much as possible of the muscle can be saved.

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FOR DISCUSSION, SEE PAGE 117

STUDIES ON THE FUNCTION OF THE PERIOSTEUM

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In 1739, the French naturalist Duhamel advanced the idea that bone grew from periosteum as an exogenous stem rises from the inner layer of the bark. However, the surgical profession and the pathologists did not generally pay attention to this subject for a good many years. For illustration, we refer to the following authors: Baillie (1795) in "Morbid Anatomy" makes no reference to the periosteum. Notwithstanding the fact that Cooper wrote concerning the open treatment of fractures, there is no mention of the periosteum in Cooper's Surgery (1819). Williams' "Principles of Pathology" deals very vaguely with ossification. In "Principles of Surgery" by James Miller (1845) ostitis and periostitis are described. With the advent of the school of pathologists instituted by Dupuytren and Cruveilhier, interest was at once awakened in the periosteum. These observers attributed to the periosteum osteogenetic qualities; and Virchow, in 1858, in his work on cellular pathology, affirms that bone-cells arise by a process of metaplasia from the periosteum. This view was reenforced by Gegenbauer, and it was not until Senn's

time that this teaching began to be disputed. Senn taught that provisional callus was the product of the periosteum, and that permanent callus was from medullary bone. Macewen, in 1887, demonstrated that bone-regeneration could take place in human fractures without the aid of periosteum (Annals of Surgery, vol. vi). In vol. 2 of Annals of Surgery (1885) Pilcher reviews Trueheart's attempt to graft the periosteum in a defect of the clavicle. Macnamara (1887) speaks of bone-grafting by the periosteum. However, the results of the last two rather conclusively show that the periosteum was not the immediate author of new bone. The more recent principles laid down by Macewen clear the atmosphere, although these are somewhat obscured by the conclusions of Haas and the experiments of Schepelmann. Albee's work does not vitiate Macewen's principles, in that in his work periosteal new bone apparently was produced by the periosteum furnishing a rallying point for the stimulated osteoblasts in irritated bone. From Ollier's work in 1860 and from the work of Barth, one concludes the periosteum not to be a material factor in the regeneration of bone-grafts. Murphy corroborated these findings. Carrel, by means of in vitro cultivations of young periosteum, with a resulting product of bone-cells, does not show that periosteum produces bone, as he has not proven that bone-cells were not included in his periosteal grafts.

It is a reasonable law in biology that all tissues are developed from cells of the same tissue. If, as has been commonly believed, periosteum produces bone-cells, it must be an exception to this general law, because it is a connective-tissue membrane, and, in accordance with this law, can only produce like connective-tissue cells, and not highly differentiated connective-tissue, such as bone.

The diversity of opinion expressed by various ob-

servers is probably due to the fact that the function of the periosteum in adult bone is slightly different from that of young bone, and vastly different from that of irritated bone. From the work of Macewen, we know that, in normal adult bone, the periosteum is a limiting membrane. In growing bone it furnishes a potential space for peripheral bone. In hyperemic or irritated bone, it serves as a refuge for the osteoblasts, and gives sufficient frame-work for new bone.

As to the relation between cartilage and periosteum, we have the following apparently paradoxical conditions:

New diaphyseal bone develops in cartilage.

Subperiosteal bone apparently develops without cartilage in a growing bone.

In drill-hole fractures, in which the periosteum may be ignored, there is practically no cartilage.

In complete fractures involving the periosteum, cartilage is present.

Bones deprived of periosteum and then completely fractured heal without cartilage.

Free periosteum does not produce cartilage.

Therefore, Macewen's principles laid down for osteoblasts hold good in cartilage cells.

From a very early period various important functions have been attributed to the periosteum which did not properly belong to it. One by one these functions have been removed, until now we are trying to take from it the last and most important rôle attributed to it, that of bone-producing power, and to reduce it to the humble position of a protecting membrane. Up to within a very few years osteomyelitis was spoken of as periostitis; and now we know that the periosteum plays a comparatively insignificant part in this important disease. It was once believed that when a bone

was stripped of its periosteum, necrosis would surely follow. It is now known that a bone may be stripped of its periosteum, and so long as its other sources of blood-supply are unimpaired, necrosis does not follow.

The late Stephen Smith advocated at one time the formation of periosteal flaps in amputation of the extremities. We soon learned that when this flap was made, the tendency was to the formation of a conical stump, and the results were not satisfactory. In the light of our present knowledge, the conical end to the bone was formed in the presence of the periosteum, not because the periosteum produced bone, but because it confined the osteoblasts to a limited space. When the periosteum is not placed over the end of the bone, it heals very smoothly without excess of bone-formation, because the excess of the osteoblasts have disappeared in the tissues.

The late Professor Sayre insisted very strenuously that in excision of the hip the periosteum should be preserved.

By subperiosteal repair we mean the formation of an involucrum beneath the periosteum. This is a common occurrence in osteomyelitis, but in a modified form may occur as a result of irritation without actual infection. Exclusive of osteomyelitis, there are references to subperiosteal repair occurring in the early literature. Celsus speaks of hastening repair in some cases of ununited fractures by irritating the ends of the bone. Physic advocated the use of setons. These are only indirect references, and do not intend to convey the idea that the authors understood the immediate result of their procedures. The army records of the Civil War abound with numerous examples of subperiosteal repair, of which I show one example. Equally striking examples are found in the University of Minnesota surgical museum, of which I show two

examples. These specimens were partially the result of infection.

More recently J. B. Murphy has called our attention to the occurrence of subperiosteal repair of bone when the segments are held apart by magnesium rods. The occurrence of subperiosteal repair is apparent in the work of Albee, and many others of our more recent writers.

Many striking examples of this phenomenon occurred in a series of experiments undertaken with the idea of discovering some osteoconductive substance other than autoplastic bone. The work of Fraenkel, Billroth, Gerster, and Elsberg has shown that inert foreign bodies in bone become encysted with connective tissue, while the work of Senn, Curtis, and Murray, with decalcified bone-chips, showed that in mere cavities bone replaced absorbable material. In our experiments actual segments were cut from the shaft of bone, and, when healing occurred, the results were entirely different from those mentioned above.

Therefore, I will detail these experiments. These are not shown for their immediate surgical value, but rather to emphasize certain principles. Various substances were used,—wooden tubes, sponge, horn of various kinds, ivory, autoplastic transplants, and blood-clot. From these series of experiments we selected examples of subperiosteal repair for the subject of this paper. The results of such experiments radically differed when the nutrient artery was cut, and where it was not cut.

In the first experiment the tibia was cut in two with a Gigli saw. The periosteum was not interfered with beyond the line of the saw-cut. The medullary canal was then reamed out, and a wooden insert made. In this case the bone was contacted with bone. Last of all, the nutrient artery was destroyed. This left the

bone with only the blood-supply from the epiphyseal artery and collateral circulation through the periosteal vessels. This was sufficient to maintain the vitality of the bone, but no well-marked subperiosteal involucrum formed. In this experiment the periosteum was left with all its properties; but the osteoblasts were not stimulated by increased blood-pressure in the medullary bone, as they would be with a normal blood-supply. This was Animal Experiment No. 227, operated on 7-16-13; *x*-ray taken on 7-28-13; again *x*-rayed on 8-28-13, and autopsy on 9-12-12. Result: non-union. Absorption of bone in region of insert. Practically no subperiosteal callus. No function.

In the next experiment the same procedure was employed except that the nutrient artery was left intact; and both abundant periosteal and endosteal bone resulted. The following experiment also shows the occurrence of periosteal bridge in bone with a normal blood supply.

A. E., No. 206. Segment removed with bone-cutting forceps. Bone held apart with Lane plate. Decalcified sponge was placed between the ends of the shaft. Operation on 6-26-13; *x*-ray on 7-10-13. Showed marked thickening of the shaft of the bone, and deposit of subperiosteal bridge. On 7-28-13 *x*-ray showed nearly completed bridge. *X*-ray on 9-23-13 showed complete bridge. In this case there was a perfect functional result, and no exudate, sinus, temperature, or other evidence of infection. Post-mortem on 9-27-13. Subperiosteal bridge demonstrated.

The next animal experiment, No. 228, shows that the periosteal bridge may extend beyond the normal confines of the periosteum. Fragments were held apart with hone-inserts and Lane plate. Operation on 9-9-13. *X*-ray on 10-13-13 shows new subperiosteal bone. This extends beyond the periosteum and begins to

bridge the gap. It looks as though the greatest production of bone had been near the middle of the defect and where there was no periosteum. On 11-18-13, bridge nearly complete. Shows also anastomosis with the fibular endosteal bone. Autopsy on 11-29-13. Shows bone to be subperiosteal.

A. E., No. 236. Defect was made in the shaft of the tibia, and a ferrule of horn was placed in this defect. The ferrule was of sufficient size to barely admit the ends of the cut bone. The ends of the mutilated shaft were held apart with phosphor-bronze plate. *X*-rayed on 10-24-13. Showed slight subperiosteal deposit. *X*-ray on 11-18-13 shows periosteal bone apparently deposited outside of the defect. X-ray on 12-9-13. Tumor-like mass of bone covering horn ferrule.

These experiments show enormous subperiosteal developments, and offer a marked contrast to the results with Lane plates. In our plate-work the periosteum plays no great part in the process of repair unless the plate causes irritation. This is emphasized by the fact that following Lane's method of repair there is practically no increase in the diameter of the bone.

Heterotopic transplants of bone produce no subperiosteal bone as shown by the following experiments:

A. E. No. 12,138. Shows a piece of femur with periosteum transplanted to the anterior muscular abdominal wall. This has undergone slow atrophy, as may be seen in the *x*-rays taken six and twelve months afterwards.

A. E. No. 13,140. Shows a piece of skull transplanted to a similar place, and undergoing a similar fate.

A. E. No. 210. A piece of epiphyseal cartilage and bone was transplanted without any periosteum, to the

anterior abdominal wall. Shows a beginning growth of bone after five months.

In all these experiments the bone was irritated and functionated, but no new subperiosteal bone resulted. This may be attributed to lack of blood-supply, or in one case to a very scant blood-supply. The bone transplanted without periosteum is of too short life to tell the final results.

A. E. No. 238. Shows result of infection and irritation. The bridge is thicker and more closely confined to the periosteum than is usual with irritation alone.

A. E. No. 12,141. Shows the result of substitution of fascia for periosteum.

The conclusions from these studies are these:

Cutting the nutrient artery prevents the formation of subperiosteal bridge. As the nutrient artery supplies medullary bone, and as the periosteum is left intact when the artery is cut, we may assume that the medullary bone is responsible for the subperiosteal bridge.

Subperiosteal bridge may fill in gaps left in the periosteum, and therefore is not absolutely dependent upon that structure even as a place of refuge.

Subperiosteal bridge is more marked where it receives osteoblasts from two sources than from one.

Fascia is a substitute for periosteum.

In heterotopic transplants, no subperiosteal bone could be produced by irritation. This is somewhat surprising in that the heterotopic experiments were conducted upon rabbits. We know that old scars in these animals frequently contain true bone, and that the periosteal device of Scheppelmann begot bone in the peritoneal cavity.

At the present day the periosteum is disregarded The present-day surgeon may be comparatively indif-

Fig. 1. Animal Experiment No. 227. Animal experiment in which the nuturient artery was cut, the bone irritated, and no subperiosteal osseous tissue resulted. Taken 12 days after operation.
Fig. 1a. Taken 43 days after operation.

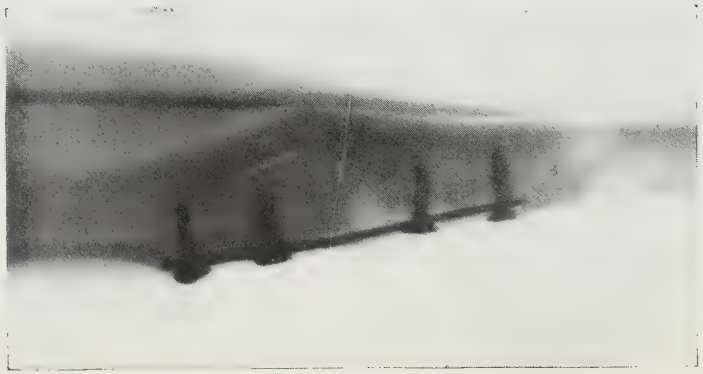

Fig. 3. Animal Experiment No. 206. In this experiment a defect in the shaft of the tibia was filled with sea sponge. Note subperiosteal bridge and complete function after three months.

Fig. 2. Subperiosteal bridge, from Museum, University of Minnesota.

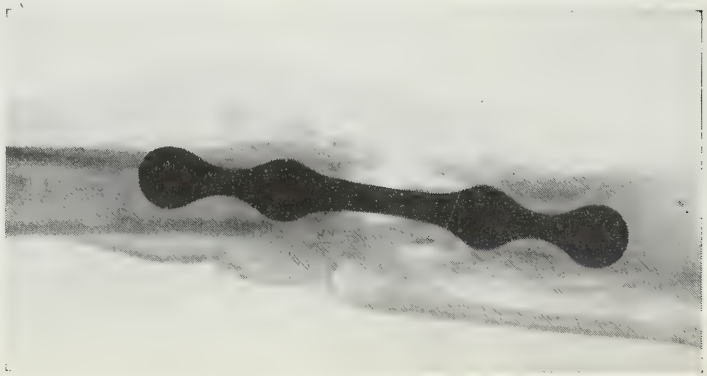

Fig. 4. Animal Experiment No. 228. In this experiment a defect was made in entire shaft of tibia, including periosteum. A piece of horn kept fragments apart. Note deposition of bone in area not covered by periosteum. *X*-ray 79 days after operation.

Fig. 5a. Animal Experiment No. 236. Defect in shaft of tibia surrounded by horn ferrule. Phosphor-bronze plate. *X*-ray at end of 25 days.
Fig. 5b. *X*-ray at end of 50 days.
Fig. 5c. *X*-ray at end of 71 days.

Fig. 6. Animal Experiment No. 12141. In this experiment the periosteum was removed from the entire circumference of the bone. A channel was cut in the bone, and continually irritated. Over this defect a piece of fascia was secured. New bone formed under the fascia, but not elsewhere.

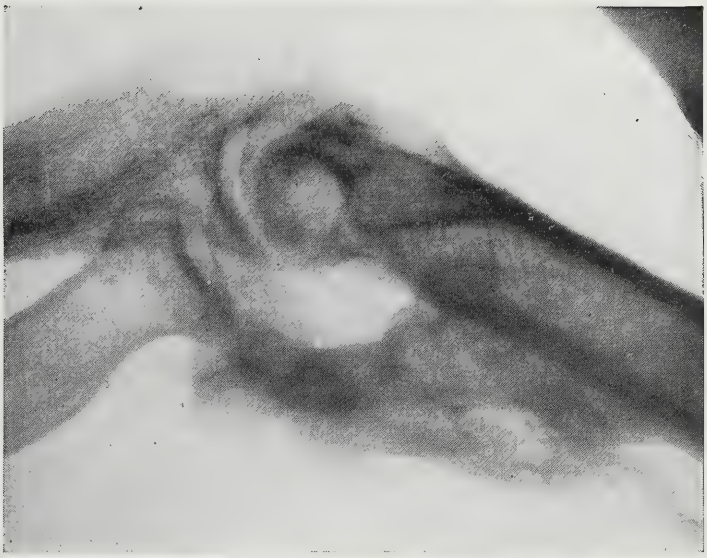

Fig. 7. Enormous subperiosteal bridge complicating fracture of the internal condyle. Operated on at the University Hospital clinic by Dr. A. A. Law.

ferent to the periosteum when operating upon bone. If it is convenient he preserves it, because the conservation of tissues is always good surgery; but, if it is not convenient, he does not hesitate to sacrifice it, knowing that it is not an essential element in the healing of bone.

Credit is due Dr. Frank Bissel for *x*-ray work.

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FOR DISCUSSION, SEE PAGE 117

SUBACROMIAL BURSITIS

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DAVENPORT, IOWA

In the discussion of the lesions of the shoulder-joint, the various more or less intimately associated bursæ received practically no consideration until after the appearance of the first paper on this subject by Codman, in 1906. Codman states that inflammation of the subacromial bursa causes more shoulder-joint disability than all other shoulder-disorders combined, including fractures and tuberculosis, a statement which a limited personal experience and observation induce me to endorse. Codman says:

> Subacromial bursitis is usually diagnosed as "brachial neuritis, periarthritis, muscular rheumatism, circumflex paralysis, contusion of the shoulder, fibrous gout, rheumatism," etc.

In the past few years a number of articles on this subject have appeared, all of them giving due credit to the work of Codman, and quoting more or less freely from his writings. Codman first called the lesion under consideration "subdeltoid bursitis," believing that there were usually two bursæ, a subdeltoid and a subacromial, the former lying between the deltoid muscle and the tuberosity of the humerus, and the latter between the tendon of the supraspinatus muscle and the overhanging acromion.

In his later papers Codman considers a single bursa the more usual; if two be found they are to be regarded as a divided subacromial bursa. Prentiss of

Iowa City insists that his dissecting-room experience leads him to conclude that this bursa is usually multilocular, an observation which further confirms the frequency of bursitis with resulting adhesions. This bursa is attached below to the fibrous expansion of the supraspinatus muscle and the greater tuberosity of the humerus, and above to the under surface of the deltoid muscle to the overhanging acromion, and coraco-acromial ligament. It is free at its periphery, that it may act out its gliding, roller-bearing function when the arm is abducted or rotated.

It is readily understood that this bursa, because of its exposed position, may be readily involved in trauma about the shoulder-joint, as in a fall or a blow. It is also readily seen that a laceration of the tendon of the supraspinatus muscle near its attachment to the greater tuberosity, would unavoidably involve the base of the bursa. The constant repetition of the same motion, as in pitching a baseball during a lively game, the pitcher being without proper training, may cause inflammation of this bursa. Any injury of the arm, or any long-continued fixation of the arm, may, it is said, also result in adhesions between the roof and the floor of the bursa. Occasionally the bursa is inflamed in general sepsis; also in gonorrhea, pneumonia, rheumatism, or tuberculosis.

Whatever be the cause of the bursitis, the resulting clinical picture will be best understood by dividing the process into three stages: In the first stage, that of acute inflammation, the rubbing together of the opposed surfaces causes severe pain, and the muscles of the shoulder-joint are on the defense, as are the abdominal muscles in the early stage of peritonitis, or the chest-muscles in pleuritis. The shoulder-joint is fixed by muscle spasm whenever motion beyond an arc of 10 degrees is attempted, motion within

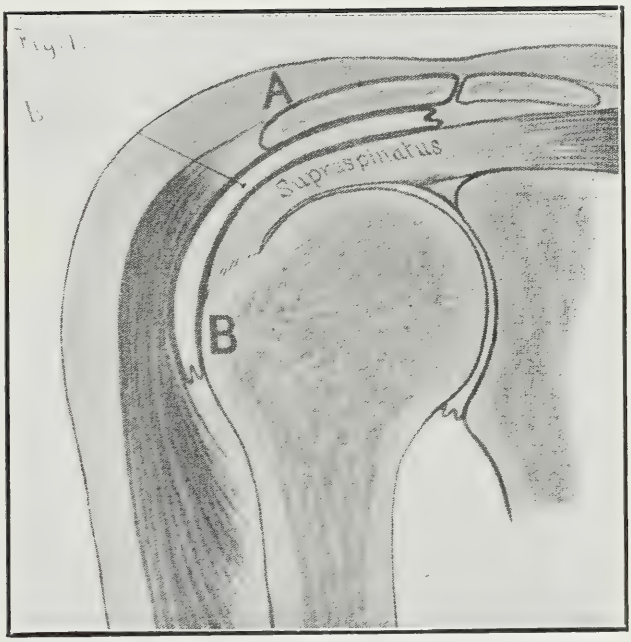

Fig. 1. Pressure beyond the acromion will cause pain.—From Codman.

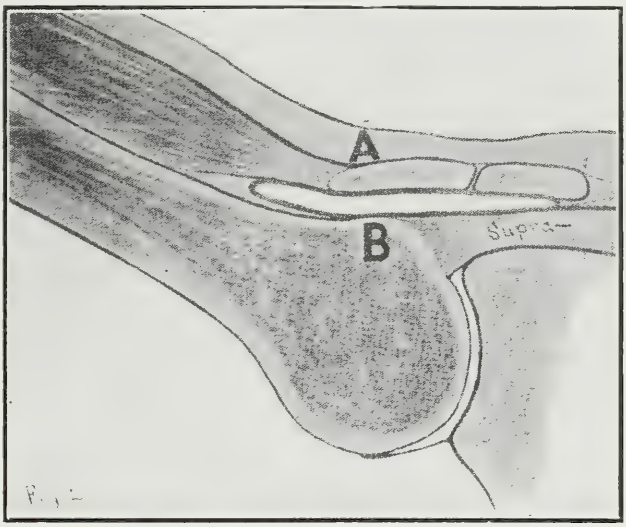

Fig. 2. On abduction the bursa slips under the overhanging acromion.—From Codman.

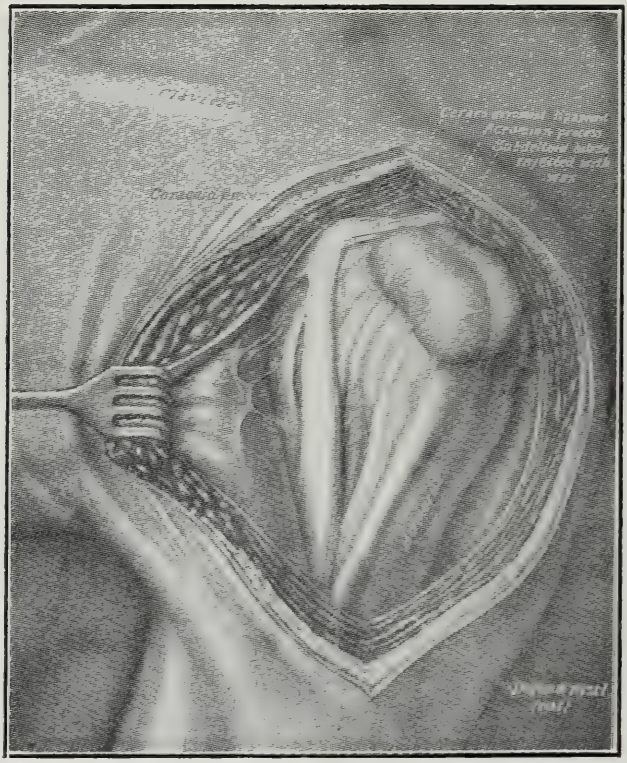

Fig. 3. Bursa injected and dissected.—From Swett.

this arc being free from pain. This limited painless motion must not be forgotten, as it differentiates bursitis from a number of other lesions, such as arthritis.

Muscle spasm and pain are very marked when the arm is abducted beyond 10 degrees. It is possible to bring the arm in front of the chest and place the hand on the opposite shoulder without pain. In fact, this position affords a sense of comfort. To attempt to button the collar at the back of the neck, involving as it does extreme outward rotation, is exceedingly painful or impossible. To button the suspenders behind involves extreme internal rotation, and is exceedingly painful or impossible. A patient may be disabled for months. Internal and external rotation and abduction beyond 10 degrees involves rather extensive movement between the opposing inflamed floor and inflamed roof of the bursa. It is difficult or impossible to find a comfortable position in bed. To lie on the involved side or on the opposite side is equally painful; while in the supine position there is a very uncomfortable dragging at the shoulder. Dawbarn calls attention to a sign which he considers pathognomonic. Finger-pressure over the shoulder just external to the acromion is very painful while the arm is hanging at the side, because the inflamed unprotected bursa is compressed. If it be possible to raise the arm the bursa slips under the protecting acromion; and this painful pressure-point disappears. Pressure at other points is free from pain. As in pleuritis, there may be an accumulation of fluid in the bursa; and it is very easily understood how in abduction this distended and tender bursa is caught between the tuberosity of the humerus and the overhanging acromion. Muscle-spasm again fixes the joint.

When the sharp pain in pleuritis is remembered, it is easy to understand how the rubbing together of the opposing inflamed surfaces of the bursa causes

muscle-spasm and fixation, as well as severe pain. As in pleuritis, there may be palpable friction. Nor is it difficult to understand how, as in pleurisy, the opposite inflamed surfaces of the bursa become adherent, and how, as the acute inflammatory process subsides, movement is less painful but limited by adhesions. In this second stage of adhesions the patient may make valiant attempts to abduct his arm to button his back collar-button, or to button his suspenders behind; but these movements are often entirely impossible, simply because adhesions do not permit the necessary movement between the floor and roof of the bursa. Nor is it difficult to understand how in the course of time these adhesions melt away, as in peritonitis, and how these same movements become possible, the pain and discomfort incident thereto slowly growing less and finally disappearing. Sometimes adhesions are entirely painless, but only in comparison to the excruciating pain of the acute stage. Codman says:

> It is during the stage of adhesions that the influence of the character of the individual on the course of the disease is most marked, because the man who can make use of his tender and atrophic muscles in spite of the soreness has a great advantage over the hypersensitive neurasthenic who has not the courage to stretch his adhesions.

If he will persist in stretching his adhesions, good results are to be expected.

In the third stage of Codman, the adhesions have given way, or are greatly stretched, and the full range of motion has returned, but with pain and discomfort still attending certain movements, as when the arm is abducted and the still enlarged bursa refuses to slip under the acromion. Sometimes the bursa is so large it locks the arm at this point; and many patients learn that by rotating the humerus outward the arm is raised without difficulty, as by outward rotation the concave surgical neck of the humerus is brought under the

acromion. Sometimes these patients think that the weather has some effect on their pet shoulder, a fact which easily leads to the diagnosis of chronic rheumatism.

The diagnosis of subacromial bursitis is not difficult when the lesion is remembered. Characteristic of the first stage are the persistence of painless motion within an arc of about 10 degrees, the muscle-spasm when movement is attempted beyond this arc, the severe pain on pressure over the deltoid muscle just external to the acromion the arm hanging at the side, and absence of pain on pressure of other points, and the painful muscle-defense on internal and external rotation of the humerus, with the locking of the joint on abduction beyond 10 degrees.

In the second stage, when adhesions are present, the diagnosis is equally easy. The more or less painful and impeded motion due to adhesions, especially on abduction, and internal and external rotation of the humerus, with the painful point of Dawbarn, are characteristic. Of course, in this stage the pain is very severe if the adhesions be stretched beyond a certain point. In the third stage the relative freedom of movement with pain when certain movements are attempted, the somewhat impeded rotation of the humerus, and the locking of the arm on abduction are quite characteristic.

Rœntgenoscopy may sometimes disclose the inflamed and thickened bursa, but is especially valuable in a negative rather than in a positive way, by demonstrating a joint apparently perfectly normal in every particular.

The prognosis is usually good if the patient has sufficient perseverance, grit, and determination to stretch the adhesions. A few months ago I saw a young man of about 30 who had but a very limited

motion in his shoulder-joint. He could not do more than bring his hands to the side of his head, and he had been in that condition for several years, an example of what may happen when the patient does not persist in stretching up the adhesions.

In the way of treatment, no attempt should be made to move the joint during the first stage, as such a procedure could only aggravate the condition. Phemister and Flint report that the removal of the fluid from the distended bursa, by means of an aspirating syringe, is followed by immediate and marked relief.

In the second stage, when the limitation of movement is due to adhesions, forced movement under chloroform may be tried, with the application of a suitable splint which will prevent the reformation of adhesions. This splint should be worn constantly for two or three days, and after this time for an hour or two daily for one or two weeks. A good method during the second stage is to increase the range of motion in the shoulder by stepping on a block of wood four or five inches high, then grasping some object just within reach, and then slowly stepping off the block. A cross-bar suspended in a closet-door by chains with one inch links is a great aid, as the bar can be raised an inch at a time, say, an inch every five or six days. The patient is to step on the block of wood, grasp the bar, hold tight, and then step to the floor. This procedure will hurt, but it will also stretch adhesions between the roof and the floor of the bursa.

The inflamed bursa has been removed with very favorable results; but some of the best surgeons in America object to this procedure, and the objection is easy to understand when the extent of this bursa and its function are considered.

CONCLUSIONS

1. Subacromial bursitis is a very frequent cause of shoulder-joint disability.

2. It is caused by traumatism or by sepsis.

3. In the first stage abduction and rotation are limited by muscle spasm.

4. In the second stage abduction and rotation are limited by adhesions.

5. In the third stage, when adhesions have been sufficiently stretched or dissolved, movement may be relatively or entirely unimpeded.

6. The bursa may be distended with fluid, and limit abduction by being caught between the tuberosity of the humerus and the overhanging acromion.

7. Treatment: In the first stage, rest; in the second stage, persistent but mild stretching of adhesions; in the third stage, continuation of movement, with possible excision of the bursa. At any stage, a bursa distended with fluid may be evacuated.

FOR DISCUSSION, SEE PAGE 117

MULTIPLE EXOSTOSES: AN HEREDITARY AFFECTION OF THE BONY SKELETON*

By Roger T. Vaughan, M. D.

CHICAGO, ILLINOIS

The points emphasized are the following: That all cartilaginous exostoses are congenital in origin, being demonstrable probably at birth, though usually not recognized by patients or clinicians until years later; that the tendency to their formation is hereditary and transmissible; that they are frequently accompanied by other developmental defects of the skeleton, some of which are characteristic of the affection; and that these tumors may be accompanied by congenital defects of other body structures.

The stunted growth of these individuals is what strikes the attention at once. They look like rachitic dwarfs, but do not have the static deformities of the long bones, as do rachitics. Their most frequent and characteristic deformity is the defect in the lower epiphysis of the ulna, which leads to a compensatory bowing of the radius, accompanied by ulnar dislocation of the hand and, sometimes, dislocation of the radial head, especially when the latter is large and deformed. Synostosis of the radius and ulna sometimes occurs, as in one of the cases shown, and also synostosis of the tibia and fibula. Genu valgum and pes equinovarus have been noted. Locking of the joints may occur with large exostoses. Sometimes the

*Author's abstract of paper read.

tips of the exostoses are provided with a bursa, which, at times, connects with neighboring joints.

As a rule, cartilaginous exostoses and enchondromata appear in the first years of life, most frequently between the fourth and sixth years. In many, or most, cases, however, the exostoses are first recognized by the physician, since they may cause the patient no symptoms. The more carefully the newborn children in exostosis families are examined, especially with the *x*-ray, the more frequently will congenital exostoses be discovered.

The fact that cartilaginous exostoses, as well as enchondromata, represent congenital anomalies of the skeleton has not been generally recognized and accepted until a comparatively recent time. Since these tumors are not usually noticed until late in youth, when they obtain a considerable size, and since the patients themselves usually consider them an acquired affection, the medical profession has been late in recognizing their congenital origin. Of late years, however, there have been a number of cases recorded in which these exostoses were noted at or shortly after birth.

The hereditary character of the affection was known long before the fact came to light that these exostoses are frequently congenital, and that more males are affected than females. Reinecke Teissier and Berard, Boyer, Fischer, Sonnenschein and Drescher, Weber, and others have described such exostosis families.

The etiological factors of these bony and cartilaginous growths are practically unknown. In several exostosis families intermarriage of near relatives has been noted. According to von Bergmann, it is a disease *sui generis,* representing a disturbance in growth of the intermediary cartilage, due to an original defective anlage. Various authors have considered that

Fig. 12. A spur-like exostosis near the lower epiphyseal line of the femur.

Fig. 13. Cartilaginous exostoses on two phalanges of the middle finger. The shadow below the finger is from a ball upon which the finger rested while being photographed.

rickets might be a factor in the origin of the affection. Doubtless numerous mistaken diagnoses have given rise to this idea, inasmuch as the deformity has been considered due to rickets, instead of to the exostoses and their associated cartilaginous and bony defects. No direct connection between the two diseases has ever been demonstrated positively. Histological examination shows that the arrangement of the cartilage cells in growing exostoses is different from their arrangement in rickets. In some of these cases a small and barely palpable thyroid has been found; in other cases there has been lack of development of the sexual function.

Multiple cartilaginous exostoses are, as a rule, a harmless affection, the disturbances in the wrist- and elbow-joints being the most bothersome. A few exostoses may, on account of their location, become very burdensome, but are rarely dangerous. Those exostoses which lock the joints may be removed. Neuralgia, paralysis, epilepsy, apoplexy, defective hearing, dystocia in labor, and rupture of the uterus, exophthalmos, rupture of the popliteal artery, with aneurism formation, and also malignant degeneration, have been described.

In view of these severe, even if infrequent complications, and the considerable and not infrequent deformities occurring in this disease, the undesirability of the intermarriage of members of exostosis families, in the light of Mendel's law, should be recognized by physicians and eugenists.

DISCUSSION ON THE FOUR PRECEDING PAPERS

Dr. Arthur Ayer Law (Minneapolis, Minn.): I had hoped that Dr. Fay, in his extremely valuable contribution to the literature of parosteal trouble, would touch upon the occasional difficulty of making a differential diagnosis between osteosarcoma and these more benign conditions. We recently discovered in the University of Minnesota clinic that it is a physical,

absolute impossibility, on some occasions, to differentiate these benign conditions from the extremely malignant osteosarcoma, which has a mortality, generally from metastases, of practically 99 per cent.

The case in point, which makes me bring up this question, was a boy, who was likewise injured in a football accident. Like some of Dr. Fay's cases, he was kicked above the left patella. We saw him a month after the accident, when he had a typical fusiform, non-inflammatory tumor, just above the patella, apparently in connection with or attached to the femur. We had a series of concentric rays taken of his femur, twenty in all I think. Dr. W. J. Mayo saw the case, Dr. James E. Moore also, and practically the entire staff of the University Hospital. They were unanimous that we could not differentiate this from periosteal sarcoma, for the simple reason that the clinical picture presented by the *x*-ray was that of osteosarcoma with feathery osteal trabeculæ in the tumor about the femur. Section and microscopical study were the only diagnostic means which would clear up the case; but we were loath to do this, except as a last resort, realizing that metastases takes place from sarcomata through the blood-stream, instead of the lymph-channels. He was finally explored, and it was found to be inflammatory, pure and simple. He had a degenerating blood-clot, with bone-formation.

Dr. Arthur E. Hertzler (Kansas City, Mo.): I saw yesterday an *x*-ray of the leg of the captain of the University football team. It shows a very large shadow of the smoky variety of Dr. Fay. It is about eight or nine inches long, and extends from the lateral side clear over to the inner side. Now, I believe if Dr. Law will keep this point in mind, that the ray-like processes in ossifying myositis run parallel to fascia and muscle fibres, while, in periosteal sarcoma, the rays are fanlike, extending from the base at the bone in all directions and not parallel to the muscle fibers, he will have no trouble in differential diagnosis.

Dr. Law: These were fan-like.

Dr. Hertzler: Arising from the bone?

Dr. Law: Apparently arising from the bone.

Dr. Hertzler: Have you got a picture?

Dr. Law: Yes; plenty of pictures.

Dr. Emil G. Beck (Chicago, Ill.): I believe that I can throw some light on the differentiation of the different conditions involving the bone. I have made a study of this, and, fortunately, have one of the pictures here with me, just by accident. I have taken different bones (of course, these are only specimens), and made stereoscopic pictures of them; and through the stereoscope you get the very fine points, the "three dimensions." The changes which are on a single *x*-ray picture very diffuse become concrete, and thus the pathological condition of diseases and tumors, etc., can be differentiated. You can differentiate a sarcoma from cyst or inflammatory condition. Take these pictures, which are marked "sarcoma," "cyst," "syphilis," etc., and study them. I have carried on this subject for three years and have quite a collection ready for publication, and I think this will solve a good many differential points. I wish I had the originals with me, but these reproductions tell different structures in different bones.

I can also throw some light on the subject of Dr. Fay. In this case, where the femur was entirely and, I suppose, accidentally removed, some of the periosteum is retained, adhering to the muscles. As it shows in this case, the periosteum was the source of the process. I have also found a good many cases of rib-resection where the reproduction occurred in the same places over again; however, we know that there is also a transformation of cells into bone, and cases have been reported where bone was not in the neighborhood of ossification of muscle, and in one of these I saw beautiful radiograms of ossification, pure bone in the penis.

Dr. Grant: What is the difference between malignant degenerative osteosarcoma and non-malignant condition?

Dr. Beck: You mean by the stereoscopic examination?

Dr. Grant: Yes; in the pathological representation.

Dr. Beck: In the sarcoma, the bone is replaced by tumor throughout. There is no outer layer of bone. It is entirely a soft structure; but in cyst there is a cavity filled with fluid or clot enclosed by a thin layer of bone. You will see the difference in the pictures. They speak for themselves.

Dr. Malcolm L. Harris (Chicago, Ill.): This is a very interesting subject brought to our attention by Dr. Fay. An article has recently been published by Küster, who has made the most extensive study of the subject with which I am familiar. He considers 107 cases belonging to this particular

variety, traumatic. There are two other varieties of the so-called ossifying myositis. In the first place, it is found that practically all the traumatic cases develop at the point where the muscle has a broad attachment to the bone; in other words, where the muscle fibres come into connection with the bone. There is no periosteum which, in any way, can play a part in the process at this point. That is important in the diagnosis. The locations are characteristic. Second, and this is very important in the differential diagnosis: This condition never has a distinct, individual growth like a tumor, sarcoma for instance. When once the foundation is laid they do not show a progressive tendency—you always see the original outline in the early *x*-ray pictures. They may appear to enlarge in successive *x*-ray pictures, but the original foundation is practically always laid early, and they do not show a progressive tendency after that time.

The tendency to follow the muscle-fiber planes, pointed out by Dr. Hertzler, is correct, and has been shown by different writers.

Another exceedingly important point bearing on treatment is, that the great majority of them disappear spontaneously if let alone. If operated on too early, they practically always return. This is shown in one case that the Doctor operated on with recurrence. So I would say, let them alone in the early stages. Massage, or anything of that kind which irritates them, tends to make them become worse, that is, more dense, not necessarily an enlargement, but denser; and there is slower spontaneous recovery. Let them alone, treat them gently, and the great majority disappear spontaneously. If any do not disappear spontaneously after a reasonable time—a few months—if the bone has acquired a density which it seems is going to persist, then it may be removed without particular danger of recurrence.

Dr. D. S. Fairchild (Clinton, Iowa): This group of cases constitutes a very important contribution to a certain branch of surgery that we shall have to deal with in the future, and for which we shall have to give an account, from the standpoint of industrial results and of claims against the physician for malpractice. Again, in the industrial insurance which the states are undertaking in various parts of the country and which will probably in the United States, as abroad, become the common practice, an explanation will now and then be demanded to determine what relation traumatism bears to the condition found.

I have been interested in the subject of Dr. Fay's paper, and I had the opportunity of adding another case to Dr. Fay's collection. A young man, about seventeen years of age, received an injury to the elbow which was diagnosed as a fracture and which was reduced and retained by the application of splints. The physician having the case in charge referred the patient to a surgeon in our community who is doing surgical practice on the commission system. An *x*-ray plate was made, and an operation upon the elbow-joint advised, similar, I suppose, to that recommended by Dr. Murphy. This treatment the patient declined to accept, and came to me for examination and treatment. An *x*-ray picture showed a well-developed bone-formation, known as *myositis ossificans*. As it was only six weeks since the injury occurred, the patient was advised to keep the injured parts in a state of rest for the present. In the past we have had occasion to examine parts where a piece of bone was said to have been splintered off, and which we now recognize as myositis ossificans.

Dr. James F. Percy (Galesburg, Ill.): In reference to the paper of Dr. Fay, I would like to report a case, but would first ask him a question, and that is, whether he has personally treated any of these cases with the *x*-ray or known of them to be so treated. Ten years ago I operated upon a woman about forty-five years of age, for a rather extensive carcinoma of the right breast. She had no signs of recurrence for eight years, and then she returned to me with pain just above the collar-bone and extensive swelling of the right arm. Examination showed that the subclavian was probably being pressed upon by a growth, and the *x*-ray revealed a bony mass coming from the body of the fourth or fifth dorsal vertebra, and extending up back of the lung and appearing inside of the collar-bone on the side from which the breast had been removed. I assumed that this was a recurrence of her carcinoma in the body of the vertebra. I had one of my surgical friends, in whose opinion I had a great deal of confidence, see her, and he gave an absolutely bad prognosis, saying she would probably live not more than two or three months. In order to be doing something, I put her on *x*-ray, and also gave her the fetal autolysates of Fichera.

That is two years ago. She has had in the neighborhood of 230 *x*-ray treatments. She seems to have a marvelous tolerance of this agent, as far as her skin is concerned. There has never been a sign of an *x*-ray burn, and we have used high tubes and in large doses, at the same time giving her the

autolysates. At the present time there is almost nothing to be seen of this growth, which I might say gave one in the beginning the idea of a cow's horn with a large base tapering off and curving a little toward the top. A radiograph at the present time shows practically a disappearance of the growth. The arm has recovered its usefulness, and there is no further pain, and the woman looks as though she were in perfect health. Now, I am wondering if this, after all, was not a case of malignant carcinoma, but was a growth of the type that has been brought out here, not only by this paper, but also by the paper of Dr. Vaughan; and it brings up the whole question of the borderland relationship of these various growths. I have in the hospital now a case that I operated on a week ago, of the type that Dr. Vaughan makes the main part of his paper.

Dr. Fay (closing): Dr. Vaughan brings out the point that there may be some question whether or not Case 3 is one of the parosteal callus-formations which we have under discussion. This question was taken up in a paragraph which I did not read because of lack of time. It was so classified because the trainer at the institution where he played football reported that he had had this peculiar "charley-horse," which did not respond to treatment. It was gradually reduced to its present peculiar form. It points towards the hand, and from its location I take it that it originated in the external attachment of the brachialis anticus. I cannot prove that this is the case, but these are my reasons for placing it in this list, and I simply show it for what it is worth. I really think that, as Dr. Harris has pointed out, these shadows may disappear.

Differential diagnosis has also been discussed. We think that we can base a differential diagnosis upon three things: First, there is the line of light between the parosteal mass and the bone-shaft, and this is as a rule very clear. In Cases 3 and 4 it is not well-marked; in the former because of the age of the process, firm fusion having resulted; in the latter because the callus saddled the bone as shown by operation. Then the outlines of the bone-shaft are clear-cut, and the bone itself appears normal, a proof that the neoformation is not sarcoma. This is, I think, shown in all of our cases. We have been working on this subject for over a year, gathering the material and imposing upon our friends by frequent discussions of it; and from this study and discussion we have come to the conclusion that such a differential diagnosis can be made.

We prefer to look upon these callus masses not as tumors, not as anything destructive, but rather as a reparative process;

and, because of the reparative nature of the process, we prefer to term them parosteal callus, that is, a callus away from the bone. While most of them are situated near a bone, they do not necessarily occur here; several typical cases in the abdominal muscles following laparotomy have been reported. Any theory which we accept in explanation of their origin, should be as applicable to the callus-formation at a distance from the bone as to that near it.

In relation to treatment: We have already said that when the mass interferes with function its removal is indicated. This interference with function was the indication for operation in our first case, but a review of the literature taught us that the recurrence probably might have been avoided had we waited until the mass was "ripe." Its growth is self-limited, and when the period of growth is past the danger of recurrence is minimal. Some authors place this time limit at approximately three months, but instead of measuring it in weeks and months it seems wiser to be guided by the pictures given us by the *x*-ray. The first patient was so eager to have the recurrence removed that he was with difficulty induced to wait the required length of time, but in this secondary operation, as in the other primary operations, we waited until the *x*-ray showed the process at a standstill and there was no recurrence. Now, whether the case which Dr. Percy has reported is one of parosteal callus or not, I do not know, and I do not know whether the *x*-ray would cure it. Many of these growths separate from the bone and remain free in the muscle; others become attached to the bone and later give the picture of an exostosis.

Dr. Corbett (closing): I think this case of Dr. Law's violated all of the rules of differential *x*-ray diagnosis between sarcoma and reparative bone. Many points in the differential diagnosis, between malignant and non-malignant bony growths, as shown by radiographs, have been given by Shenton and others. These principles were, in my experience, not always confirmed by the *x*-rays of experimental bone lesions.

Dr. Vaughan (closing): I have here a Röntgenogram of a single isolated exostosis, which I should like to pass around to show you how much like this third case of Dr. Fay's it is. The thing which would differentiate a myositis ossificans from a cartilaginous exostosis would be to find a cartilage cap at the tip of the latter. The Röntgenogram in Case 3 of Dr. Fay's is so suggestive of a cartilage cap that I am quite inclined to doubt the diagnosis.

I had a case like Dr. Law's two years ago. A strike-breaker was hit on the arm with an iron bar by a striker. He developed a large hematoma and came to the hospital. At the end of a week, the hematoma became very hard, seemed to be increasing in size, and we thought very seriously of an acutely developing periosteal sarcoma; but when, at the end of three weeks, the *x*-ray showed tiny spicules of bone developing all through this mass, which spicules did not radiate as do those of a periosteal sarcoma, the diagnosis of traumatic ossifying myositis was not difficult. Not knowing any better, I tried to cut it out, and had what to me was the astonishing experience of having a recurrence in about three days, it being then a little larger than it was before. The railroad surgeon, whose case this was, then suggested that it would be a good thing to cut out the entire brachialis anticus, because this mass appeared chiefly confined to it. Before undertaking a so radical procedure, however, I looked up the literature and found that a very great number of surgeons reported the same experience, in fact it seemed that everybody who had tried to operate on an acute, traumatic, ossifying myositis had had a recurrence, and, as Dr. Harris says, all the surgeons, particularly the Germans, seem agreed that traumatic myositis should be left alone until it spontaneously disappears, or has been quiescent for a long time.

AVULSION OF THE SCALP

Arthur Ayer Law, M. D., F. A. C. S.

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The blood-spattered pages of American history, up to the time the frontier disappeared, show that surgeons were not unfamiliar with scalping accidents. The American Indian, unlike the head-hunting savages of Borneo and the Philippines, took alone the scalp of his victims as one of the trophies of his success in warfare.

Since the first recorded case by Downs, in 1838, surgeons have learned to think of scalping as those cases which were the result of accidental injury,—the cases of "industrial scalping," so called. Usually the patients are young women working in factories whose hair becomes entangled in a revolving shaft, which violently jerks the scalp from the head.

These cases are not extremely rare. Davis, in an exhaustive study of the literature, collected 92 instances of complete avulsion of the scalp, 81 of which were caused by machinery, 7 by Indians, and 4 by other forces. Of the 81 industrial scalpings, 80 were women and the other a Chinaman with a queue.

The student of this condition is impressed with the great length of time these dreadful wounds take to heal, even though they are given the benefit of modern treatment. In the earlier records, cases are reported which took from two to four years to cicatrize; and one instance is cited by Syme which was

still unhealed after eight years. An analysis of twenty-five reported cases treated since the technic of skin-grafting became perfected, still shows that an average of ten months' treatment was necessary to cover the denuded heads.

The anatomical structure of the cranium and its coverings explains, in large measure, this protracted healing period. The scalp is usually torn away through the subaponeurotic layer, leaving the pericranium bare, or part of this latter membrane may itself be taken. The outer layer of the skull, unlike most other bones, is nourished largely by the blood-vessels of the diploë; the pericranium plays but a minor rôle in its nutrition, being very thin and but slightly vascular, therefore granulations are a long time in forming on it.

When the skull is laid bare there are frequently necrosis and exfoliation of the outer table; and granulations form in the vascular diploë and lift up the thin layer of bone above, which can readily be scaled off. When granulations do form on the denuded bones, they begin at the suture-lines, where the pericranium as the intersutural membrane dips between the bones to join the dura. These granulations then very slowly spread over the bony tables finally to coalesce and form a new scalp of granulation-tissue. Epitheliation must, perforce, begin at the skin edges, and spread from them over the entire dome of the head.

Is it any wonder, then, that, prior to the age of antisepsis, asepsis, and skin-grafting, many of these patients died of sepsis, meningitis, or erysipelas? When we study the blood supply of the scalp, and note the emissary veins and the intercommunication of its arteries and veins with those of the diploë, dura, and dural sinuses, the marvel is that all of these cases do not die of meningitis or cerebral abscesses, for they are all primarily septic.

In most of the reported cases of complete avulsion the integument was torn where it was thinnest, at the level of the brows, or even from the eyelids themselves, the direction of force being exerted from before backwards, the superciliary ridges acting as the wedges which caused the cleavage. Fourchard has demonstrated this in experimental avulsions upon the cadaver. Generally the entire scalp to the level of the ears is torn away and occasionally one or both ears as well are lost. The defect usually ceases at the hair-line on the neck. Cushing observes that it is liable to run down on to the center of the neck in a V.

The scalp usually separates from the temporal muscles, leaving them intact; the bellies of the occipito-frontalis, however, are generally taken with it. Hemorrhage is frequently free, and occasionally has required ligation of the blood-vessels. Shock is sometimes profound; but, if death occurs primarily, it is due to hemorrhage or to other injuries.

Primary pain is rarely severe; but, several hours later, it may be excruciating, and after granulations are well established there is little or none. These patients frequently suffer from the prolonged suppuration. Nephritis may result, and they are wasted and anemic.

Elaborate reports detail the efforts of many men to cover the large defect present. Keen states that success has attended skin-grafting upon the bare bones of the skull. This seems incredible when we consider the necessity of an adequate blood supply to keep the grafts viable.

Repeated efforts have been made to graft alien, or "zoödermic," grafts upon the granulating areas, with 100 per cent of failure, for, while a few observers report that lamb's skin apparently "took," it invariably sloughed inside of two weeks.

Thirteen surgeons have stitched the avulsed scalp back over the defect; and with but one exception—and that is doubtful—report complete sloughing of the scalp. Others report cutting the shaved scalp into strips and grafting it as such, with general failure.

Hundreds, even thousands, of Reverdin grafts have been used on the same individual with ultimate healing after a long period of time. Plastic sliding operations are reported where a flap from the neck or side of the face, with pedicle attached, has been used to cover the brow or part of the cranium.

Where avulsion was not complete and only the vertex was denuded, after the method of Volkman, four concentric flaps from the edges of the scalp were cut, slid across the wound, and united, making a cross of scalp, the defects between the arms of which were grafted in other ways. Whole thickness grafts, after the method of Wolfe-Krause, were used by Davis with complete success.

It is by Thiersch's method, however, that the greater number of wounds have been healed; and autogenous grafts have given the best results. The "iso" grafts do well for a time, but ultimately they are liable to break down in ulcerations, probably the new blood supply over the skull being less perfect than in other parts of the body where approximately 50 per cent of these grafts take.

In the case the writer reports, in conjunction with Thiersch grafts, amniotic membrane, both from the sac and the cord near the fetus (after the method advised by Stern), was used in three different instances to cover small granulating areas, two of which definitely took and the third sloughed. Carrel's success with the transplantation of refrigerated tissues which have been kept in paraffine for several days or longer, would suggest that this method be tried with whole pieces of scalp or skin.

While a large number of surgeons exhausted their ingenuity in the treatment of this condition it seems to have escaped their notice that a frontier army surgeon, as early as 1769, advocated a procedure which greatly simplified and shortened the time of treatment. James Robertson (quoted by Felix Robertson, in 1806, in the *Philadelphia Medical and Physical Journal*) advised boring the outer tables of the skull with a shoemaker's awl to let the granulations through from the vascular diploë. This procedure was suggested to him by an unknown French surgeon, and was a frequent practice when Indian warfare made the injury common, but has been lost sight of as a surgical procedure since, as there is no record of its use until Snèvè, in 1889, reports using this operation to cover a small defect due to a burn over the left parietal bone.

When the writer's case presented itself, the reported histories of the long-delayed granulations, exfoliation of the outer tables, and suppuration with its attendant complications, suggested that this method be tried.

His case in detail is as follows:

H. H., female, single, age 19, admitted to my service at the University Hospital, from Osakis, Minn., March 28th, 1913. She gives the following history: On March 19th a belt running a cream-separator kept slipping from the drum of a line shaft. She got up near the shaft to put the belt on again; her hair caught in the whirling shaft, and her scalp was jerked off her head, letting her fall to the floor. She did not faint, but bled profusely. Her father was present, and, under telephonic instructions from the doctor, put the scalp back on her head, and applied hot cloths to it. The doctor arrived an hour and a half later, and stitched the scalp in place.

When admitted to the hospital, nine days later, the scalp was still on her head. Over the bare bones of the vertex it was dried like rawhide, and was necrotic around the edges, having shrunk to two-thirds its original size. The wound

commenced in front above the left eyebrow, angling across the forehead and dropping down on to the right eyebrow; on the right temple it dipped to the level of the zygoma, skirted the ear, hugged the hair-line around the back of the head, ran an inch above the left ear, and at the outer edge of the left orbit met another linear wound, which ran down on to her cheek and below the lobe of the ear, and made a detached triangular flap of scalp, ear, and integument, which had been stripped up and was attached only by its base. The whole head area was bathed in pus.

She was septic, and had a hemoglobin of 66 per cent. Her head was dressed with wet gauze wrung out of an iodine solution. On the sixth day, after the dried scalp was softened, it was removed, revealing the temporal muscles already granulating; but the entire vertex and frontal and occipital regions were bare of pericranium, muscle, or granulation-tissue, save for small isolated islands of granulations along the lambdoid suture and on the forehead.

This great wound was daily dressed with wet iodine gauze, and rapidly became reasonably clean. On April 12th, twenty-four days after her injury, there were healthy granulations on the forehead and over the temporal muscles. Under ether anesthetic these areas were completely covered with Thiersch grafts taken from her thighs. The bare tables of the skull, after painting with tincture of iodine, were drilled through the diploë with a one-eighth inch drill, the drill holes being one-half inch apart and fifty-seven in number. The grafts and drill holes were covered with narrow strips of gutta-percha tissue, which, in turn, was covered with a voluminous gauze dressing saturated with normal salt solution.

Drill holes invading the vascular area of the diploë are an added menace at first, for they are then absorptive; later, when the granulations form, they are excretive. This was illustrated by the girl's rapid rise of temperature from normal on the day of operation to 104.6° on the second and third days thereafter. Her eyes, nose, and cheeks became edematous and dusky; and she was isolated under suspicion of erysipelas. She ran a temperature for ten days, when it again dropped to normal. All the grafts took; but it was not until the twentieth day that granulations appeared in the drill holes. After that they grew rapidly, mushroomed out of the holes and over the bony tables finally to coalesce and cover in the entire denuded area six weeks after the operation. In the meantime the girl developed a mild case of diphtheria, and

Fig. 1. Photograph taken March 30, 1913. Posterior view showing the "V" at the nape of the neck. Part of old scalp still remaining.

Fig. 2. Photograph taken May 20, 1913. Other side of head, 26 days after drill-holes were made through the outer table of the skull, showing granulations cropping through and different stages of development, showing thin veil over the granulations on the occiput, due to grafted amniotic membrane.

Fig. 3. Photograph taken August 15, 1913. Showing patient with entire scalp of transplanted skin.

was isolated until June 3d, when the remaining area of the head was covered by Thiersch grafts from her thighs and legs. These took well, save a small area below the occiput, which was rubbed off in her sleep. This was again grafted on June 21st; and at the same time a Wolfe-Krause hair-bearing graft was taken from her mons veneris, set into a wound created in the new skin over her right eyebrow, and stitched there by horsehair sutures. This graft lived, as did the hair; but about two-thirds of the latter dropped out a month later, which was probably due to lack of adequate circulation for the hair-bulbs, which experience is in keeping with that reported by Enz and Lotheissen. On July 12th a small area, one by two inches, over the vertex had broken down, and was grafted with amniotic membrane, with success. On July 20th a small area back of the right ear had broken down. This, as well, was grafted with amniotic membrane; but did not take, and was finally healed under scarlet red. On July 29th two small areas, which had broken down, were again grafted with amniotic membrane and healed.

On August 25th she was entirely healed, and left the hospital August 29th, or four and one-half months after the primary operation. At the present writing, except for a small area on the vertex, the new skin is movable, perspires, and is sensitive.

Admitting that the average time of treatment for these cases is ten months, this simple operation suggested by the unknown traveling French surgeon during the troubled days of the Indian wars, should take its place as an accepted surgical procedure which is both time- and life-saving.

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DISCUSSION

Dr. George M. Gray (Kansas City, Kas.): I wish to relate an experience that I had myself in connection with a case similar to the one Dr. Law has shown by the screen, the accident happening in the same way by the young lady getting the hair caught in the shaft. I was not as fortunate as Dr. Law in the outcome of this case. The granulations, which appeared in the course of weeks, were exuberant all over the scalp. There was constant loss or dripping of blood from traumatism of the granulations, I suppose by the head moving upon the pillow. However, the case was prepared for a skin-grafting, a day or two before the expected time of skin-grafting, by the application of gauze with glycerine. She was taken to the operating-room, and upon attempting removal of this dressing, there was a very profuse hemorrhage from these granulations; in fact, by the time that I had removed that dressing she was in profound shock simply from the hemorrhage occurring from these granulations, and any attempt at that time to prepare the granulations for suitable skin-grafting by the Thiersch method was out of the question. She was returned to the ward in profound shock simply from the hemorrhage which occurred from these granulating surfaces, and died within a few hours from the shock.

Dr. Leonard Freeman (Denver, Colo.): Dr. Law has called attention in his interesting paper to the method of drilling holes in the external table. It is a most excellent method, but I think it should be emphasized more strongly than he has seen fit to emphasize it that it is a dangerous method, unless the external table be thoroughly sterilized, and that is a difficult thing to do in the presence of suppuration. In the case he described, he attempted to do this by painting with tincture of iodine; but, as the rise in temperature shows, he did not succeed. There have been cases of infection from this pro-

cedure which have been fatal. Might it not be well to make larger holes in the external table than the holes made with a shoemaker's awl—by enlarging these holes by some sort of biting forceps or by using a trephine? Thus the drainage would be better. The drainage through so small a hole as that made by a shoemaker's awl is hardly adequate under septic conditions.

Dr. Law: I would suggest, Dr. Freeman, that someone, I don't know who it is, has suggested taking off the entire outer table. I emphasized, I believe, the danger of this procedure, but the danger from the suppuration due to prolonged healing is so much graver that I believe we are justified in using this method.

GAS-BACILLUS INFECTION, WITH REPORT OF CASES

W. D. Haines, M. D.

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As coroner's physician some years ago it became my privilege to hold post-mortems on a few bodies dead of gas-bacillus infection. Careful inquiry during the prosecution of this work, and in the development of the forensic part of the inquests, failed to discover a single recovery following a well-authenticated case of this type of infection.

One of the constant post-mortem findings is the so-called "foamy liver." This condition, while indicating the presence of the gas bacillus, does not necessarily imply that the bacilli became active during the life of the patient. The "foamy liver" may be found in subjects dead of other causes wherein gas bacillus played no part in the cause of death; that is to say, "foamy liver" in such instances is a post-mortem change, pure and simple. Infection, in such instances, may have taken place shortly before death ensued, at a time when the inhibitory forces of the liver against germ-invasion were held in abeyance. The multiplication of the bacilli and the development of their sugar- and proteid-splitting powers occurs before the animal heat leaves the body; thus "foamy liver" means the presence of the gas bacillus only, and, like many other post-mortem findings, becomes valuable in fixing the causation of death when correctly interpreted by the forensic examiner.

The intestinal tract is one of the normal habitats of the gas bacillus, hence invasion of the cellular structures through a gastric, duodenal, or intestinal ulcer, may take place, producing unusual symptoms, confusing the doctor, and destroying the patient.

I have in mind a patient convalescent after a herniotomy whose abdomen became greatly distended, and the walls crackled like parchment. The patient developed a high temperature, became unconscious, and died on the third day. This man's post-operative history was smooth; and he was practically ready to leave the hospital when this unfortunate circumstance took place. Unfortunately, a post-mortem examination was prohibited by the friends; but, clinically, I have ever regarded this as a death induced by gas-bacillus infection.

The disease appears as a local phlegmonous inflammation, and is characterized by extensive serous exudate and the presence of hydrogen gas in the connective-tissue spaces. Pressure gangrene and profound general toxemia are frequently associated with these clinical manifestations. The prodromal symptoms are headache, backache, excessive thirst, and general malaise. The period of incubation in one of the cases observed by the writer was forty-eight hours; and in the other case the time could not be definitely fixed, as the patient could not give an intelligent account of himself.

The bacillus aërogenes capsulata being anaërobic in character, the successful management in a given case of gas-bacillus infection will depend more upon free exposure of the affected tissues to atmosphere than upon any local or internal medication. Unfortunately, this line of treatment is not always possible of application, if some organ is involved rendering free incision inexpedient. Pressure necrosis, general

toxemia, and death have hitherto followed in the wake of gas-bacillus infection.

CASES

In the month of September, 1905, I saw Mr. W., a powerfully built laborer of 50, in consultation with his family physician, Dr. Scheerer.

The patient had been ill but two days at the time of the consultation. Previous to this time he had had good health, and had been working regularly for several months.

Four days previous to my visit the patient injured himself while trying to raise a heavy weight by the aid of a crowbar. The fulcrum slipped, and the free end of the bar, in its rapid descent, struck the patient in the right groin, causing considerable contusion of the deeper structures and a slight abrasion in the skin immediately over the external inguinal ring. No heed was given to the injury; and the man continued at his work for the remainder of that and the next succeeding day. On the evening of the second day following the injury, the patient complained of an intense frontal headache, and noticed on retiring that his scrotum was somewhat swollen.

The following morning, forty-eight hours after the receipt of injury, Dr. Scheerer found the patient with a slight increase in temperature and pulse, and still complaining of headache. The scrotum was enormously enlarged, edematous, and of a dark purple color. I saw the patient two days later, ninety-six hours after injury, at which time the scrotum looked not unlike a large-sized egg-plant that had been gnawed in numerous places by rodents.

There was a foul-smelling, blood-tinged serous discharge exuding from each little break in the skin, which was accompanied by an occasional gas bubble. The process was wholly confined to the skin and connective tissue of the scrotum. Multiple incisions and a saturated solution of alcohol with salicylic acid were tried locally, while supportive and symptomatic treatment was given internally. The sloughing was extensive, but never extended beyond the scrotal tissues. The man was convalescent at the end of a month, and finally fully recovered with a very fair though much contracted scrotum.

The clinical diagnosis was confirmed by the laboratory report as gas bacillus.

I shall not take up your valuable time in relating details of the second case. Suffice it to say that a slight wound of the skin covering the right forearm was followed by an infection which was characterized by a general toxemia and an enormously swollen arm. On incising the skin and subcutaneous tissues, gas bubbles, bloody serum, and pus appeared. Salicylic acid in alcohol was injected into the spaces and applied locally. This patient was desperately ill, and his physician, Dr. Tischbein, despaired of his life upon numerous occasions.

He recovered after many months with a badly misshapen arm, but with sufficient function to permit him to follow his vocation, that of a molder.

The after-history of this man was extremely interesting. Within one year from his recovery he had an abscess in the region of the shoulder of the arm which had been infected; this, however, was not supposed to have any connection with his previous illness. Subsequent developments may well cause us to question this opinion, for the patient succumbed to a kidney lesion some six months afterwards.

Autopsy revealed a badly disorganized left kidney, which contained much detritus and some free pus. Numerous germs were present, but one in particular. A large non-capsulated and faintly Gram-positive bacillus was regarded by the laboratory man as a subvariety of the gas-forming group.

DISCUSSION

Dr. Byron B. Davis (Omaha, Neb.): Inasmuch as I have had two experiences with the gas-bacillus infection, I thought possibly it might be proper to relate them. The first one occurred about twenty-two or twenty-three years ago, before we really knew much about this gas bacillus. It was a simple injury. A brakeman received a trauma between the thumb and the index finger. He came under my observation five days later with an arm that was practically gangrenous, crackling in every point, the edema and crackling extending over on the chest-wall, and with a temperature, I have forgotten how high, but it was very high, and with a very rapid pulse. In that case, I immediately amputated at the shoulder-joint, laid everything wide open, kept the wound open, made two or three incisions over the pectoralis major. The man, for some rather unaccountable reason, recovered.

The next case was probably twelve or fifteen years ago. A man met with a penetrating wound of the knee, not a very

serious wound, and was brought in from two or three hundred miles out of Omaha. The condition at the time he came in was the same condition of gas in the subcutaneous tissues, not only throughout the entire leg and thigh, but also up over the abdomen. This case, in spite of all we were able to do (we did an amputation, but whether it was advisable or not I do not know), died in thirty-six hours, and by that time the edema had extended up over the chest-walls, over the abdomen, and he looked like a monstrosity. We did not isolate the germ in either case, but in this last case we injected some of the fluid from the wound into a guinea-pig and got a typical condition, such as is produced by the gas bacillus.

Although it is an infrequent condition, I feel that the treatment of opening up the tissues very thoroughly and getting the air to them as far as possible, is undoubtedly the thing. In these cases I question now whether the best thing is amputation. I think the best treatment is many incisions, in order to open the wound up and allow the air and oxygen to get in contact with the tissues. In the last twelve or fifteen years I have not seen a single case. It is rather rare.

Dr. Carl E. Black (Jacksonville, Ill.): I would like to mention two cases. My experience is confined to five cases of gasbacillus gangrene, two of which were unusually interesting. One case was a boy and one a girl, each about twelve years of age. Both were school-yard accidents and both compound fractures of the forearm. The gas gangrene began in one case at the end of forty hours, and in the other at about the end of the third day. Within twenty-four hours in the boy's case from the time of beginning of the gangrene, and in forty-eight hours in the girl's case, each developed tetanus; and we had as a complication of the gas gangrene typical tetanus, which was treated in the usual way by the antitetanic serum. In both cases amputation of the arm was made high up, and the antitetanic serum was used in maximum doses. Both children made good recoveries. I mention them because I have inquired of a number of colleagues, and so far have found no one who has had the experience of gas-bacillus gangrene complicated by tetanus.

Dr. Haines (closing): I have very little to say in closing, but to call your attention to a rather unusual sort of condition, which you may encounter in your work. If you do not recognize it early, and deal with it radically, you are going to lose your patient. As was said in the paper, it is not always pos-

sible to deal with these infections radically. If you have a perforation of the intestines, the physical manifestations of gas-bacillus infection may not appear until after the death of the patient. I saw one man with an abdominal gunshot wound and several perforations, who succumbed to shock, hemorrhage, operation, and so on, in the course of twelve or fourteen hours. I made a post-mortem probably twenty-four hours later, at which time the body was enormously distended—a post-lethal development. Now, he had had his infection prior to the time he died, but the signs appeared after death.

The Doctor speaks of a period of incubation of forty hours in one case. I reported a case where it was forty-eight hours before there was any swelling noticed. In another, the man was unconscious, and I could not get a history.

Dr. Davis spoke about checking up his work by taking some of the detritus, and injecting it into a guinea-pig. This is very virulent, and consequently you will find inoculation and the entire syndrome of symptoms following very quickly, whereas if you take an emulsion of almost pure culture, and inject it into the veins of a guinea-pig, he will survive.

I agree with the Doctor that amputation is hardly the proper treatment. I think free incision, being very bold about getting well into the cellular tissue, as the infection is largely confined to cellular-tissue spaces, is the best treatment. We felt probably we had found something in the salicylic acid or alcohol, which we applied, but as a matter of fact it was just letting in the air, as the bacilli do no damage in the presence of oxygen.

SYMPOSIUM ON JACKSON'S VEIL OR PERICOLIC MEMBRANE

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THE SIGNIFICANCE OF THE JACKSON VEIL

Daniel N. Eisendrath, M. D., and
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CHICAGO, ILLINOIS

At the 1912 meeting of the Western Surgical Association the discussion as to the true significance of the Jackson veil or pericolic membrane showed that the question was far from being solved. A committee composed of Drs. J. Rilus Eastman, Howard N. Hill, and D. N. Eisendrath was appointed to furnish the Fellows of the Association with blanks in order to note their findings at operation and to report the same at the present (1913) meeting.

The principal phases of the present symposium should be, according to my conception of the question, (a) whether the membrane or veil described by Jackson in his first paper[1] is always a pathological structure; (b) whether it is always a normal structure; or, finally, (c) whether it is a normal, i. e., a developmental, membrane, which may undergo pathological changes and give rise to conditions which cause clinical symptoms.

The committee asked each Fellow to report his observations in as concise a manner as possible; and, as chairman of your committee, I shall open the subject for discussion by a report of my observations (a) in the living, during operation; (b) in the

[1]Surgery, Gynecology and Obstetrics, Sept., 1908.

human cadaver; and (c) in the human fetus. I have noted our findings in 25 cases operated on during the last four months at the Michael Reese Hospital. All of these were operated on on account of symptoms of acute or chronic appendicitis, with the exception of Case 25, which will be described separately in Group 4. As a matter of convenience one can divide the observations made during operations into four groups as follows:

Part a. 1. The membrane corresponds in every detail to the normal parietocolic fold of the fetus or adult cadaver, even though there is ptosis of the cecum.

2. The membrane is thicker and less vascular than in Group 1, and may cover a portion of the cecum and appendix.

3. The pericolic membrane (parietocolic fold) fuses at its lower border with the fold of Treves or the two membranes (parietocolic and Treves) are present separately.

4. The parietocolic fold has undergone pathological changes with resultant kinking or the production of a double-barreled shotgun deformity of the ascending colon (Fig. 23).

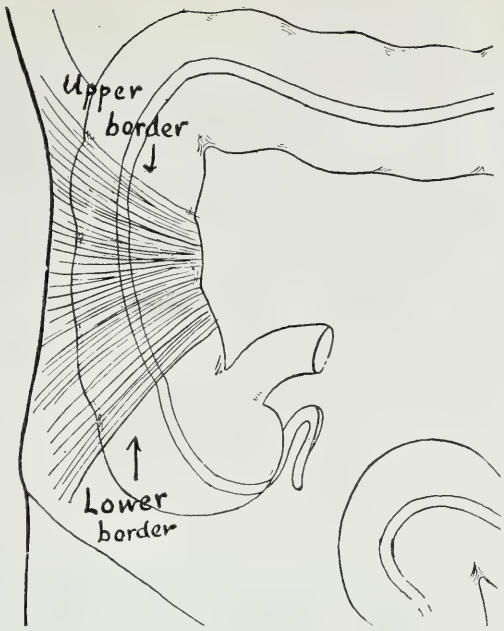
Group 1. (See Fig. 1).—These cases (15 in number) all showed a fine veil-like translucent membrane, which was reflected from the parietal peritoneum, and then passed across the front of the ascending colon, ending gradually at the anterior longitudinal band in some cases, while in others it extended across the entire front of the ascending colon, either fusing with the peritoneum of the mesocolon or with the omentum near the hepatic flexure. The upper and lower borders could be readily picked up, the upper being at the level of the hepatic flexure and the lower from 1 to 1½ inches above the termination of the cecum. In one case, the cecum and ascending colon

were so mobile that the cecum and appendix (Fig. 2) were found quite loose on the right side of the true pelvis, between the bladder and the lateral pelvic wall. Even in this case the membrane, although much longer than when the cecum was in the right iliac fossa, showed the same upper and lower borders and other characteristics of the pericolic membrane. Age seems to make no difference in the distribution and other features, except that the membrane seems a little more vascular than later in life. The ages in this first group varied from 12 to 28 years. In none of these cases had any angulation of the cecum or ascending colon occurred. The condition of the appendix amply explained the clinical symptoms. The mobility of the cecum exerted no influence upon any of the characters of the membrane. In none of these cases was the membrane disturbed. In some the membrane showed many fibrous strands, and deposits of fat.

Group 2.—In two cases (Fig. 3) one operated on for a general peritonitis due to a perforated appendix, and the other a case of tuberculous peritonitis, the membrane was very thick and fibrous. But even in these cases there was no constriction or angulation of the colon or cecum; and the upper and lower borders were the same as those in the first group. These two cases are of interest in showing that under certain conditions the pericolic membrane may only resemble the normal structure in its anatomical distribution, but be many times thicker and quite opaque. One can readily see how such a membrane can undergo either circumscribed or diffuse cicatricial changes with subsequent kinking and other forms of obstruction in the caliber of the colon.

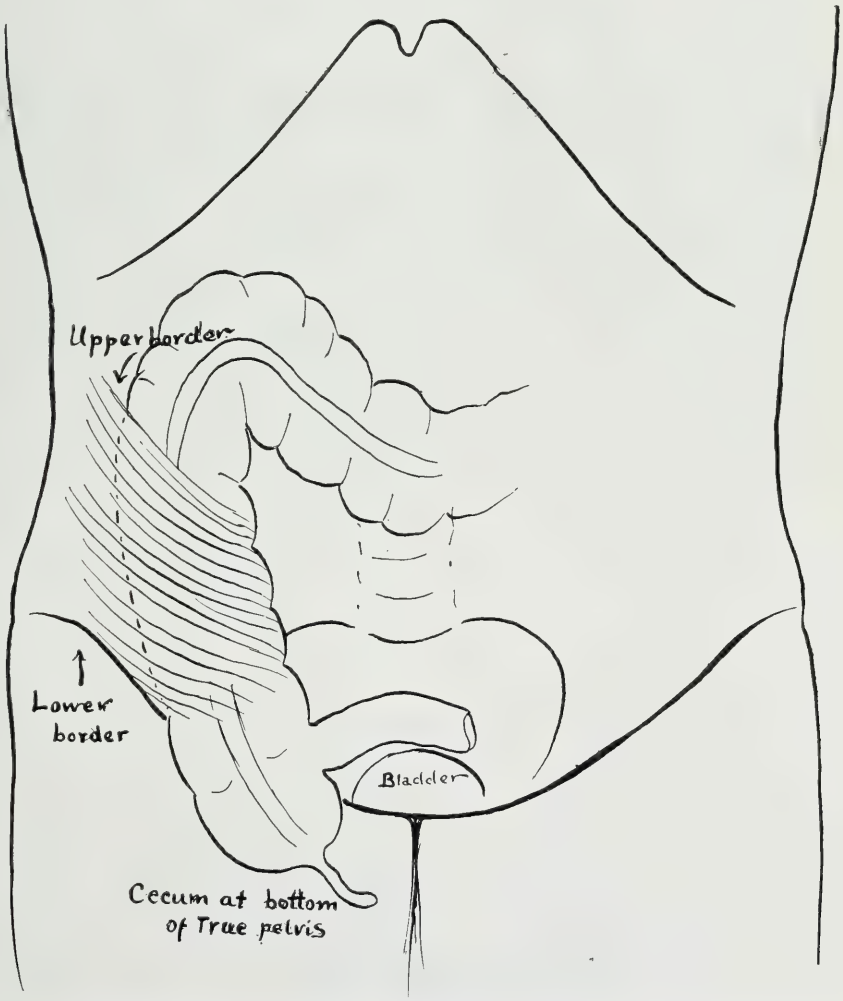
In two other cases of this second group, the upper border of the parietocolic fold was the same as in the first or "normal" group, but at its lower end it covered a portion of or the entire cecum. In one case

Fig. 1.



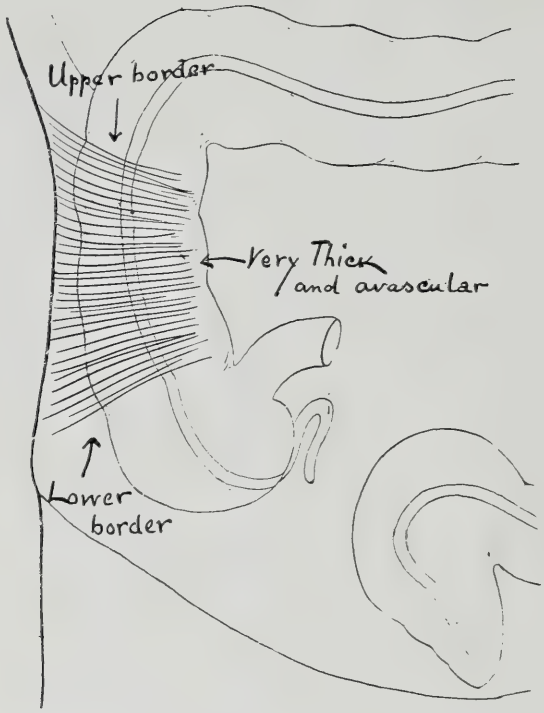
Sarah M., aged eighteen. Chronic appendicitis. Upper border at hepatic flexure, lower border one inch above end of cæcum. Not very vascular membrane extending across colon (ascending) to fuse with mesocolon on inner (mesial) side.

Fig. 2.



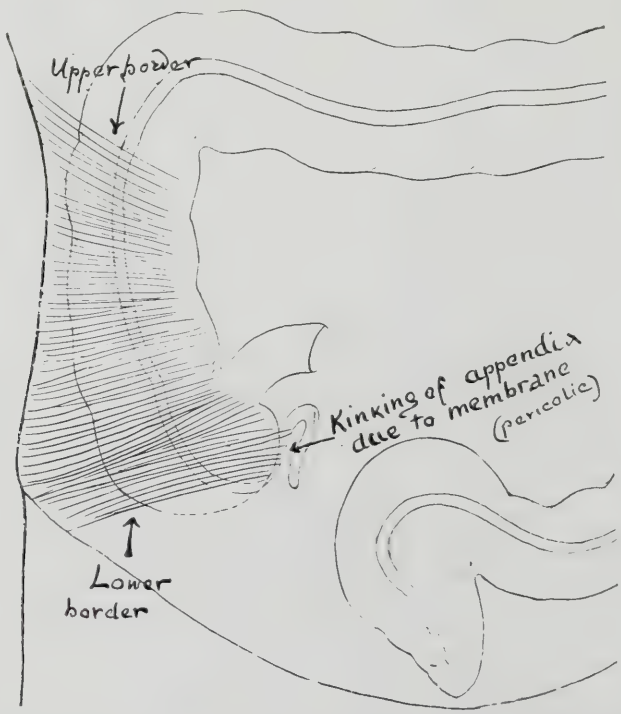
Jane Y., aged twenty-eight. Chronic appendicitis. Ptosis of cæcum and ascending colon into true pelvis. Cæcum and appendix lying at bottom of true pelvis. Typical pericolic fold with upper border at hepatic flexure and lower just above cæcum.

Fig. 3.



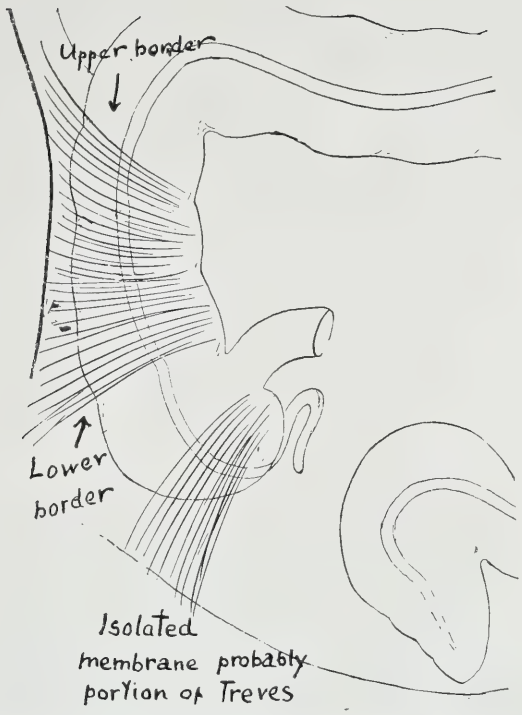
Herman E., aged fifty-seven. Tuberculous peritonitis, evidently primary of appendix or cæcum. Pericolic fold had typical upper and lower limits but was avascular, one inch in thickness, and studded with miliary tubercles. Upper border at hepatic flexure and lower border 1½ inches above end of cæcum.

Fig. 4.



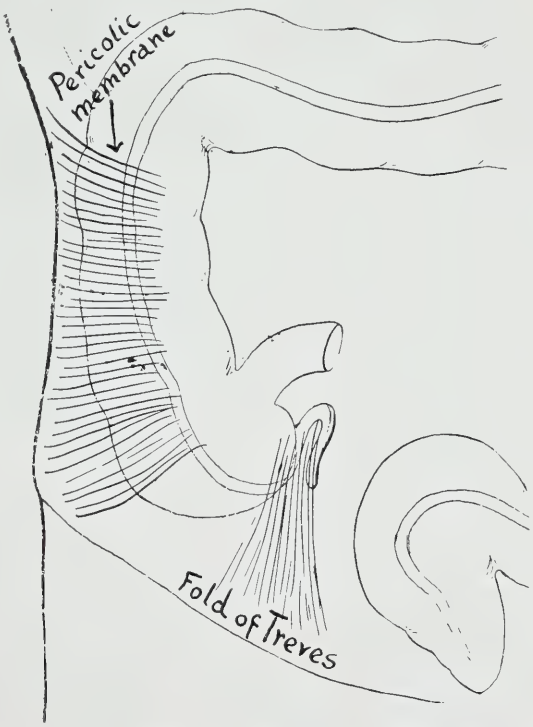
Esther B., aged eighteen. Acute appendicitis. Upper border of pericolic fold at hepatic flexure. Appendix folded upon itself close to cæcum by extension of pericolic fold downward over proximal third of appendix, causing kinking of appendix, which was greatly enlarged and inflamed distal to the kink.

Fig. 5.



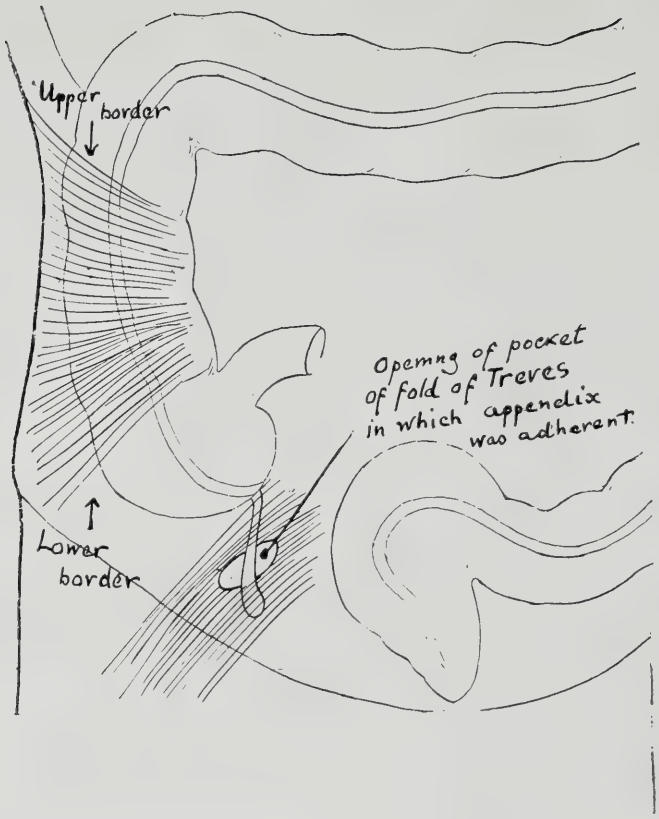
Jennie M., aged twenty. Chronic appendicitis. Cæcum very movable. Pericolic fold, upper border at hepatic flexure, lower border 1½ inches above end of cæcum, extends entirely across ascending colon. A fine membrane (probably part of a fold of Treves) covered cæcum at its lower end.

Fig. 6.



Harvey S., aged twenty-five. Chronic appendicitis. Upper limit of pericolic fold at hepatic flexure. Membrane extremely vascular and thick in places, fibres directed downward and inward and extends across ascending colon to anterior longitudinal band or slightly beyond it. Separated one-half inch at lower end from fold of Treves, which is well marked and forms several pockets.

FIG. 7.

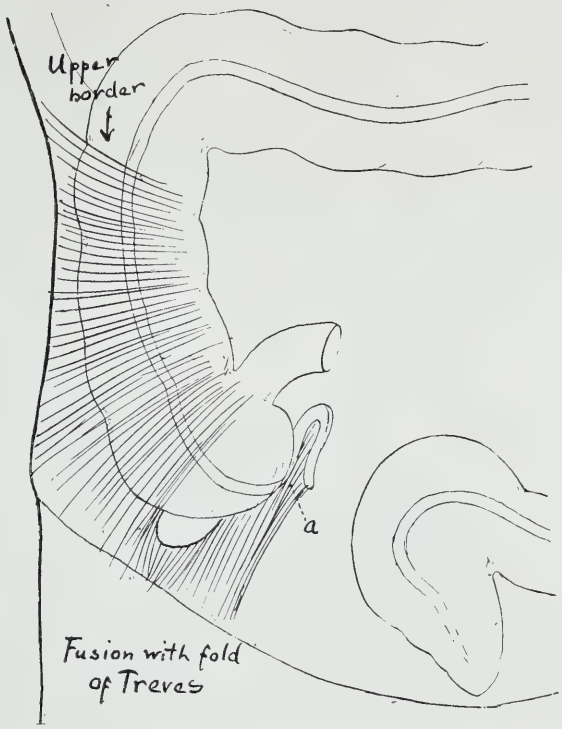


Sam C., aged twenty-eight. Chronic appendicitis. Well marked pericolic fold extending down and inward from hepatic flexure at its upper border to just above cæcum and across to anterior longitudinal band. Appendix lies in pocket formed by fold of Treves.

FIG. 8.

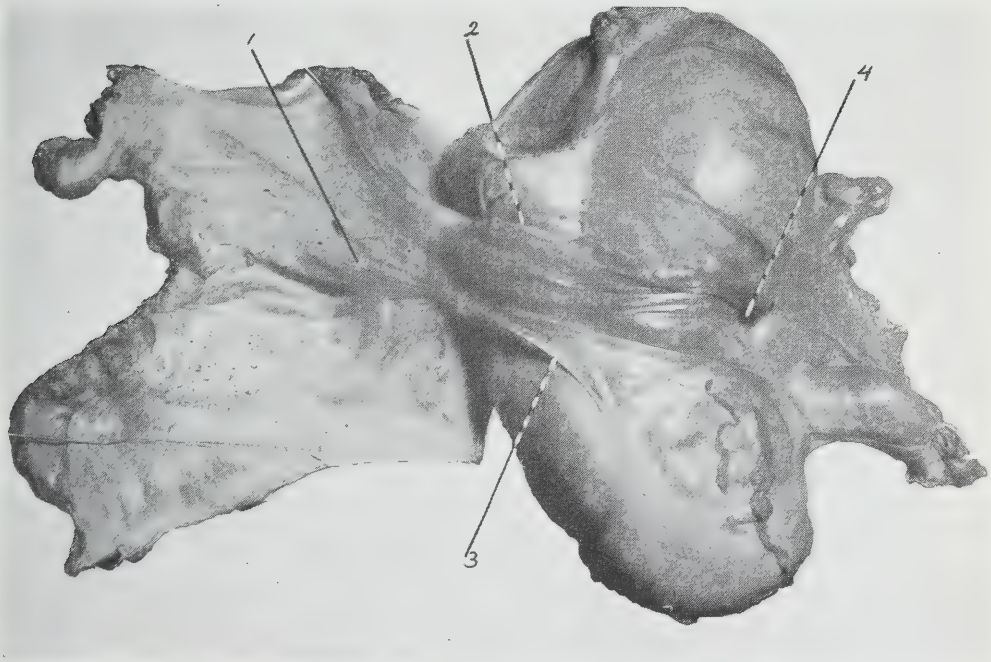
Pearl M., aged twenty-five. Pericolic fold—upper border indistinct, becoming thinner and more veil-like toward cæcum where it blends with fold of Treves which covers end of cæcum and appendix. Pericolic fold quite vascular and contains much fat.

Fig. 9.



Benjamin E., aged twelve. Acute catarrhal appendicitis. Cæcum slightly movable. Distinct pericolic veil or membrane continuous with parietal peritoneum on outer side and extending inward to mesocolon on inner side of ascending colon. Membrane fuses below with fold of Treves, covering entire cæcum and ascending colon. Fold of Treves becomes continuous with meso-appendix at *a*.

Fig. 10.

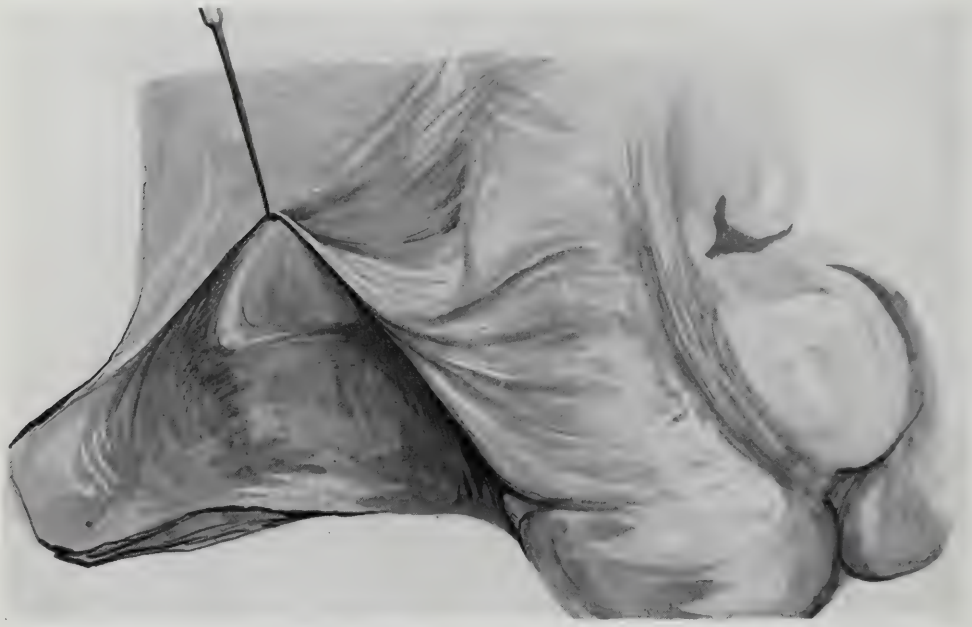


Photograph (not retouched) of ileocolic region of female adult cadaver—died of cancer of stomach, without metastases. 1, reflection on parietal peritoneum of fold of Jonnesco (Jackson veil or pericolic membrane); 2, upper limit of fold or membrane; 3, lower limit of fold or membrane (1½ inches above end of cæcum); 4, inner limit of fold or membrane (fuses with mesocolon on inner side of ascending colon).

Fig. 11.

Wash drawing of specimen taken from cadaver of adult. Cause of death, stab-wound of lung. *UB*, upper border of parietocolic fold; *LB*, lower border of parietocolic fold; *O*, fusion of upper part of parietocolic fold with omentum.

Fig. 12.



Lower end of parietocolic membrane shown in Fig. 11—lifted up to show reflection from abdominal wall.

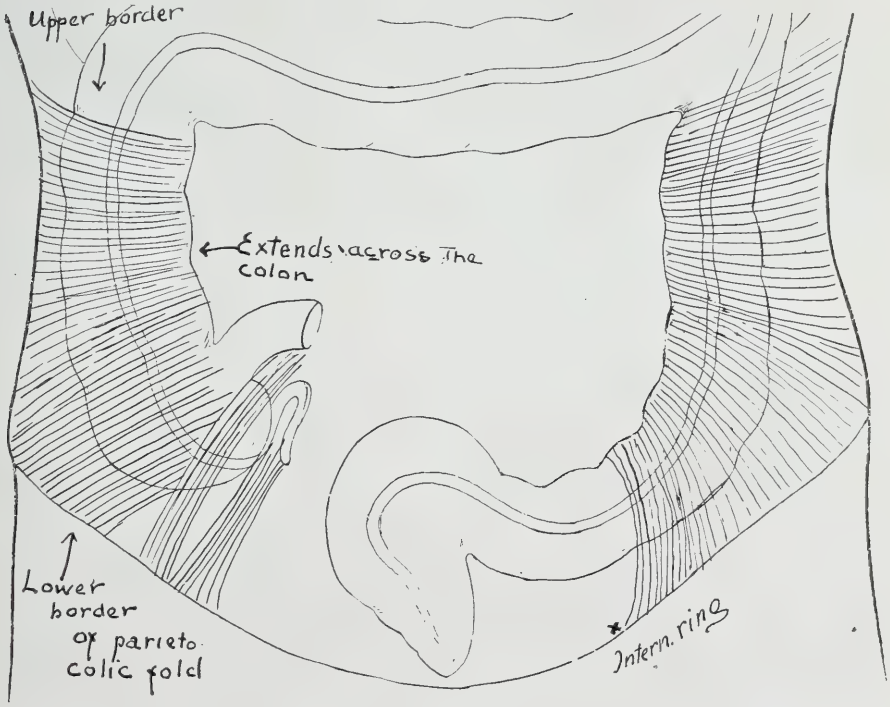
Fig. 13.

Parietocolic membrane causing double barrel shotgun deformity of ascending colon.

Fig. 14.

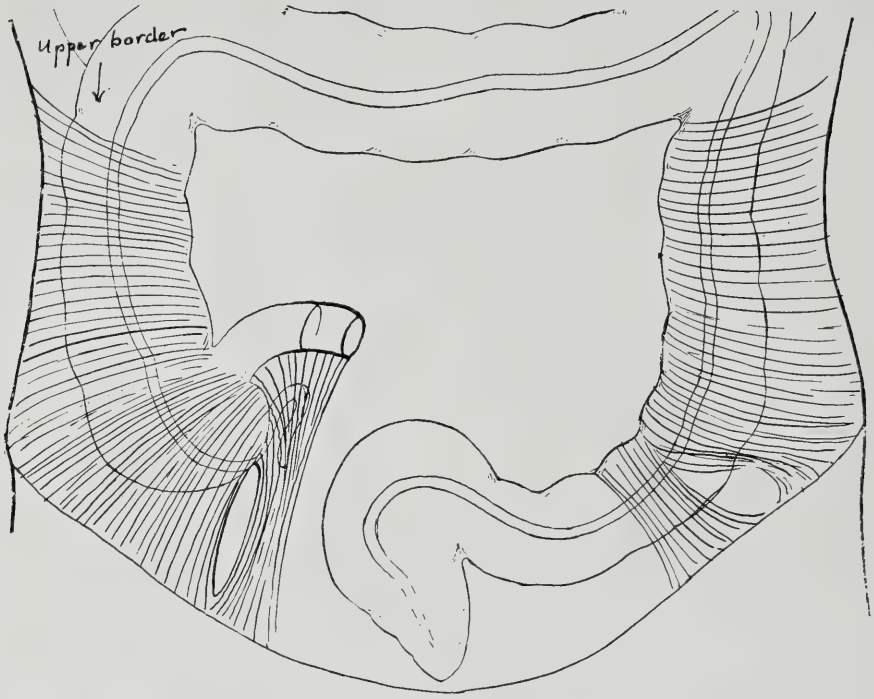
Left sided fetal parietocolic membrane. *U*, upper, and *L*, lower borders.

Fig. 15.



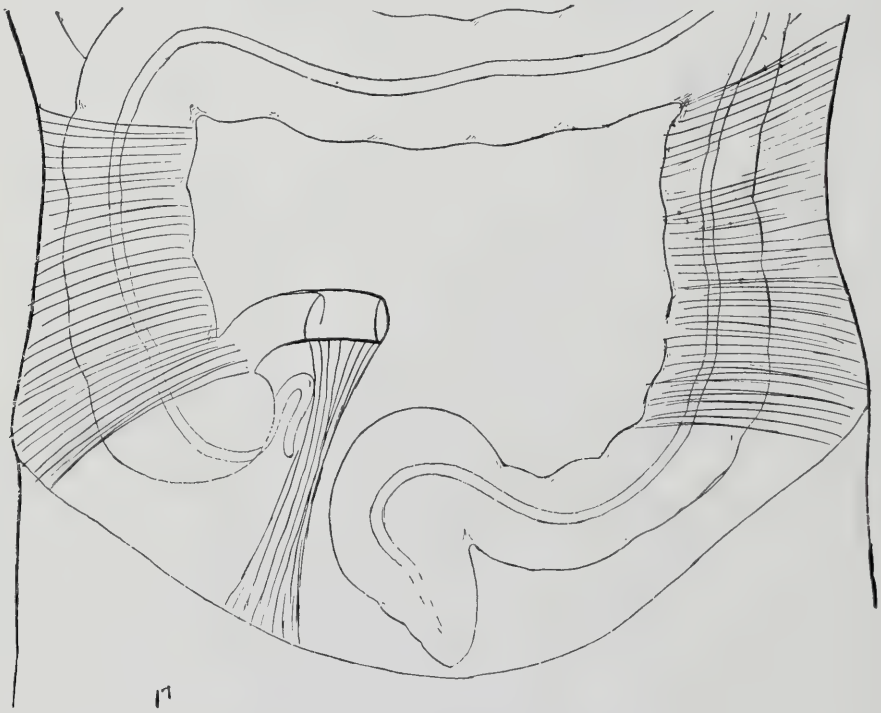
Fusion of folds of Jonnesco (parietocolic), fold of Treves and of Reid (genitomesenteric) on right side and fusion of left parietocolic fold and genitomesenteric fold of Reid on left side (from six months' fœtus).

Fig. 16.



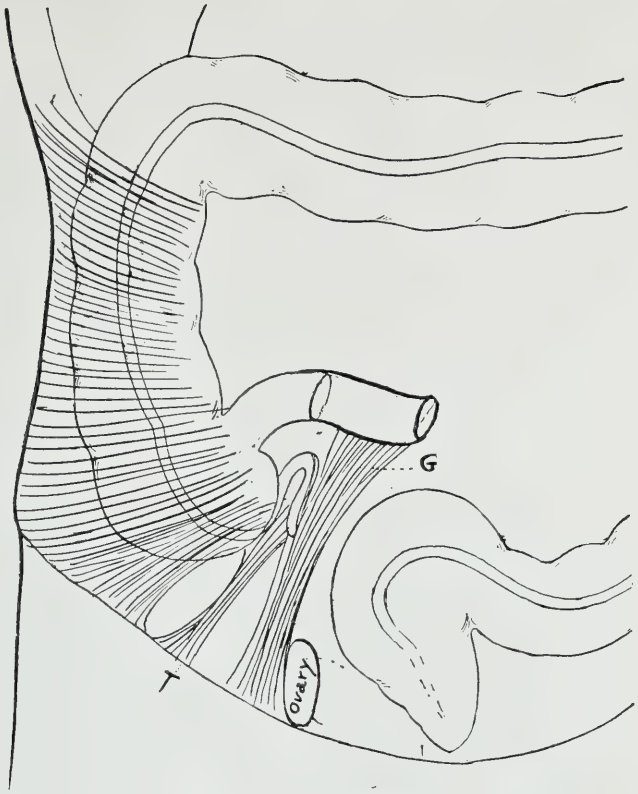
Fusion of parietocolic fold, fold of Treves, and genitomesenteric fold of Reid on right side and fusion of parietocolic fold on left side with genitomesenteric fold of Reid. This latter fusion is the rule in female fœtus.

Fig. 17.



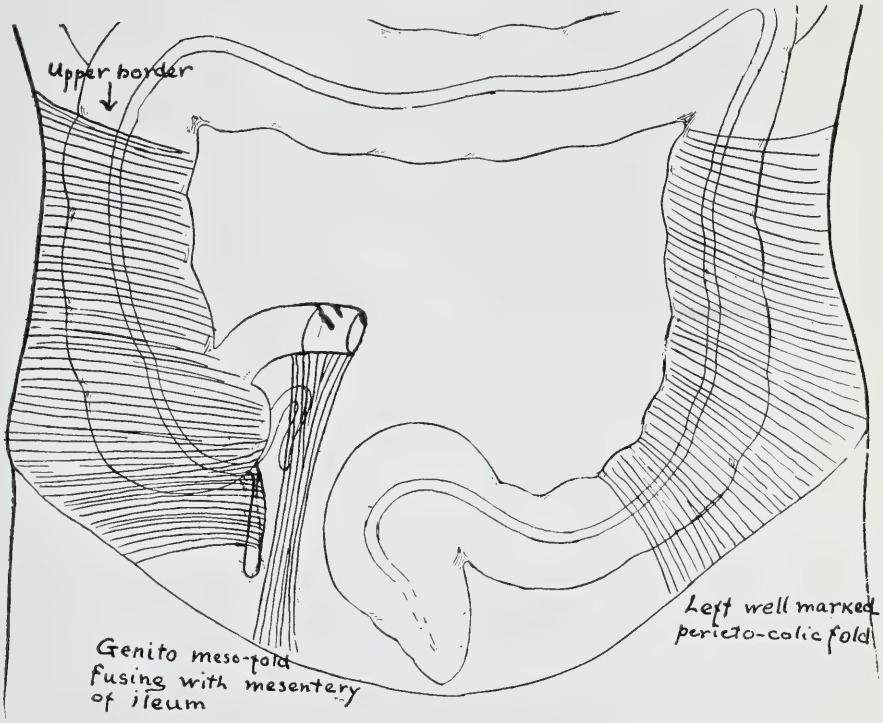
Well marked left and right parietocolic fold.

Fig. 18.



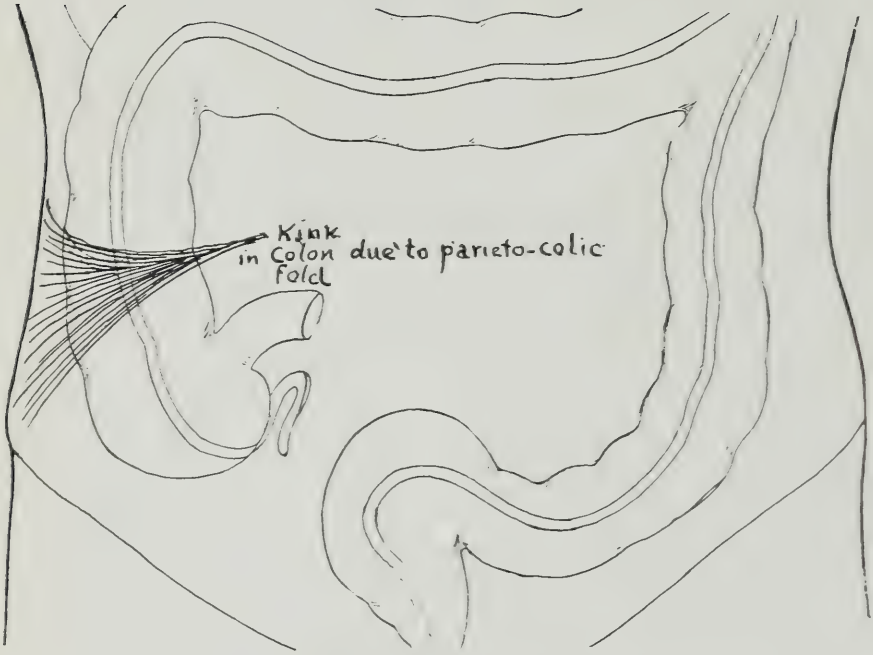
Fusion of fold of Treves and parietocolic fold.

Fig. 19.



Poorly developed parietocolic fold on right side with undeveloped fold of Treves. The genito-mesenteric fold on right side fused with mesentery of ileum. Well marked left sided parietocolic fold with upper border at splenic flexure and lower border at junction of sigmoid and descending colon.

FIG. 20.



Parietocolic fold producing a distinct kink in ascending colon.

(Fig. 4) the membrane caused a distinct kinking of the appendix with resultant dilatation of the lumen and other changes distal to the kink.

Group 3.—There were 6 cases in this group, all operated on on account of symptoms of acute or chronic appendicitis. In the first three cases (Figs. 5, 6 and 7) the parietocolic fold (pericolic membrane, or Jackson veil) and the bloodless fold of Treves were so well developed that one could distinctly note where the pericolic membrane (parietocolic fold, Jackson veil, or fold of Jonnesco) ended, and the fold of Treves began, the two being separated by a free space varying greatly in width. The parietocolic fold had all of the characteristics of the cases regarded as "normal" and placed in Group 1. The bloodless fold of Treves covered the lowermost end of the cecum or this and the appendix. In the case shown in Fig. 15, the appendix was quite firmly ensconced in a shirtwaist pocket-like niche formed by the fold of Treves. Before Eastman[2] called our attention to this action of the fold of Treves, I had regarded such appendices formerly as lying in a pocket of adhesions. In the cases shown in Figs. 8 and 9 the lower border of the parietocolic fold could be seen to fuse imperceptibly with the bloodless fold of Treves, the two covering the cecum or appendix, or both. The fold of Treves in one of these cases fused distinctly with the mesentery of the appendix. As in Groups 1 and 2, one could not find any evidences of a kink or other interference with the lumen, etc., of the colon or cecum.

Group 4.—In this case the patient was operated on for chronic appendicitis in 1908, a note at that time being made that "the ascending colon showed a well-marked Jackson veil." She returned three years later with symptoms of interference with the movements of

[2]Surgery, Gynecology and Obstetrics, April, 1913.

the colon, so well described by Jackson in both of his papers. A second operation showed that, as a result of a cicatricial contraction of a portion of the veil formerly covering the ascending colon, a "double-barreled shotgun" deformity had resulted. The cecum was greatly dilated on the proximal side of the contracted area; and an anastomosis of the transverse colon and cecum was done. I shall refer later to the significance of this personal clinical experience as showing that in some cases the veil may undergo pathologic changes, and cause kinking and other forms of acute and chronic intestinal obstruction.

Part b. Cadaver observations.—In several recent autopsies the pericolic membrane was studied within a few hours after death, and the results are shown in Figs. 10, 11 and 12.

The objection may be raised that not a sufficient number of subjects have been dissected. There is no doubt in my mind but that this method would be a most fertile field for a future investigation, but my own few findings corroborated those observed during operations in such a manner that I thought them to be of sufficient value to add as confirmatory evidence.

In Fig. 10 is shown the abdominal wall adjacent to the cecum and ascending colon removed intact and photographed, without retouching the photograph. The specimen shows distinctly a reflection of a fold from the parietal peritoneum to the ascending colon, quite independent of the peritoneum lining the abdominal wall on the outer side of the ascending colon. The upper border of this parietocolic fold (fold of Jonnesco, pericolic membrane, or Jackson veil) is at the level of the upper third of the ascending colon, and the lower border about one and a half inches above the end of the cecum. There is a distinct pocket between this membrane or fold and the peritoneum of the

lateral abdominal wall. The membrane contains quite a little fat, and extends across the colon to fuse with the mesocolon on the inner side of the ascending colon. Its fibers are directed downwards and inwards. The ileocolic region, illustrated in Figs. 21 and 22, was not adapted for a photograph, but was most conscientiously drawn from the fresh specimen to show the pericolic membrane. The membrane was much thinner than in the specimen shown in Fig. 20, and contained a number of fine vessels.

Its distribution, upper and lower borders, corresponded accurately to the description given by Hall of Jackson's first case. It extended from the hepatic flexure to a point about one and a half inches above the cecum, and fused on the inner side of the upper part of the ascending colon with the right end of the omentum. (0 in Fig. 11.)

In Fig. 12 the artist has attempted to show how the membrane can be lifted from the parietal peritoneum of the abdominal wall.

In one of these cadavers the cause of death was a carcinoma of the stomach without peritoneal metastases. In the other, the cause of death was a stab wound of the lung. Both specimens show different stages of the pericolic membrane. In one (Fig. 10) it is firm, but not fibrous, and contains much fat. In the other it is extremely thin and translucent. In both, the ascending colon and cecum were normal in caliber and appearance.

Part c. Examination of fetus. (See Figs. 14 to 31, inclusive.)—We have examined and made notes of our findings in 10 cases. The ages varied from 6 months to full term. The results were far more uniform than those reported by Dr. Eastman, who found the parietocolic fold in 5 of 28 fetuses. We have found it in nearly every dissection of cases where the rotation

of the cecum was complete. It had the same limits as in post-uterine life, but the membrane itself was extremely thin and contained a number of fine capillaries, which were directed downwards and inwards. In some of the dissections there was a fusion of the parietocolic fold on the right side with the fold of Treves and the genitomesenteric fold of Reid. The latter was quite constant on both sides, especially in female cadavers. In several specimens the parietocolic fold fused with the fold of Treves at only one point, the remainder showing a pocket-like gap, in which the appendix laid. One fetus showed imperfect cecal rotation, with an absence of both the parietocolic fold and the bloodless fold of Treves. The genitomesenteric fold was well developed, however.

Fig. 20 was sketched from a fetus where a localized thickening of the right parietocolic fold caused a distinct kink of the ascending colon. The suggestion offered by Eastman that a thickened and cicatricial genitomesenteric fold corresponds to the ileopelvic band of Lane, causing kinking of the ileum, seems to us a very plausible one.

One of the most interesting facts which our examination of fetal cadavers revealed was the constant presence of a parietocolic fold on the left side, corresponding in every particular to the same structure on the right side. Fig. 13 shows such a membrane drawn from a full-term fetus. In some of the specimens (six in number) the upper border was at the level of the splenic flexure, and fused imperceptibly with the left end of the omentum. The lower border of this left pericolic membrane was at the junction of the descending colon and sigmoid. (Fig. 23.) In several specimens there was a fusion of the left parietocolic fold with the genitomesenteric fold of Reid. The point of reflection of the left pericolic membrane from the parietal peritoneum was the same as on the right

side; and it passes across the descending colon to unite with the mesocolon. Dr. Summers had called my attention to similar observations made by Gray and Anderson in 1912 and published in the form of a monograph.

SUMMARY

From our observations during operation, from dissection of cadavers, and, finally, from examination of 10 fetuses, we believe that certain conclusions can be drawn:

The parietocolic fold of Jonnesco, synonymous with the pericolic membrane or Jackson veil, is a reduplication or fold of peritoneum, which is constantly found during fetal and post-natal life. This membrane corresponds in every way to the description given by Hale of Jackson's specimens in his two principal papers, published in 1908 and 1913, respectively. It is a fine translucent membrane, which varies greatly in vascularity. In some of our cases there were only a few fine capillaries, while in others the membrane was extremely vascular. The upper border of this right-sided pericolic membrane is almost invariably at the level of the hepatic flexure; and its lower border from one to one and one-half inches above the lower end of the cecum. In some of our cases the lower border either extended a little further down and covered the entire cecum and a portion of the appendix, or fused with the fold of Treves. In the great majority of cases this fusion did not occur. The vessels of the pericolic membrane are, as a rule, directed downwards and inwards. In two of our cases (see Group 2) the membrane was as thick as ordinary cardboard, and showed practically no vessels. In the remainder of the patients the membrane was very thin. It extends inwards across the front of colon to the attachment of the mesocolon, and either fuses with the peritoneum

covering the latter or fuses with the omentum along the upper third of ascending colon. These may be called normal cases. The membrane being a persistence of a fetal structure under no circumstances should be stripped off. Such a step would result in leaving an extensive denuded surface.

From the examination of our specimens and from observation in the living, we believe that the genito-mesenteric fold of Reid is the forerunner of the ileo-pelvic band of Lane, and bears the same potential relation to the Lane kink that the pericolic membrane bears to possible kinks of the ascending colon. That such constrictions occur one can no longer doubt. The case reported in Group 4 affords ample proof of the rôle which the Jackson veil may play in the production of acute and chronic obstruction of the ascending colon and cecum. We are not prepared to state at the present time what causes this change in the pericolic membrane from an innocent persistent fetal structure to the production of a distinct pathological entity. In our opinion Jackson, Pilcher, Gerster, Flint, and others have rendered a great service in calling attention to the various sequelæ of such pathological changes in the pericolic membrane.

One of the chief objects of this investigation, we believe, has been fulfilled by calling attention to the fact that there are two distinct types of pericolic membrane: (1) those which are innocent, and (2) those which may cause mechanical interference with the function of the colon. Each case must be judged upon the operative findings; and one is not justified in saying that every pericolic membrane requires interference. The majority are perfectly normal structures.

Our examinations of fetal cadavers confirm those of Gray and Anderson, that there is a left parietocolic fold, which corresponds in every detail to the same

structure on the right side. It is a constant finding in the fetus; and no doubt search for it in the future during operations on the left side of the abdomen will confirm these fetal observations.

The treatment of the pathological conditions due to the right pericolic membrane must depend on the findings in the individual case.

FOR DISCUSSION, SEE PAGE 179

PERICOLIC MEMBRANES

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Anomalous peritoneal folds, wherever and whenever found, notwithstanding their variations in macro- and microscopic appearance, are, with respect to their nature and origin, probably quite closely related. The frequency of anomalous pericolic folds in the fetus, and the fact of their very common presence in young children, indicate that, whatever the fundamental pathological process which causes them may be, this process frequently becomes operative in embryonal life; however, the actual manner of the formation of such membranes is not made clear by such observations. It is probable that in many instances at least the same pathological condition leads to pericolic-membrane formation in the fetus and the adult, and that reaction to irritation, with or without consequent fusion or adhesion, underlies anomalous pericolic-membrane formation in practically all cases. The extensive form of Jackson's membrane almost covering the ascending colon, if formed, as Dr. C. H. Mayo suggested, by the descending caput coli impinging against a crescentic reflection of loosely applied peritoneum, doubtless owes its existence to adhesion between the descending cecum and subhepatic mural serosa. The writer has shown how other forms of pericolic membranes which appear in the fetus are the result of fusion between cecum and mural peritoneum before the occurrence of normal cecal torsion. Concerning pericolic membranes which

arise during adult life, it seems fair to suspect, in view of experimental evidence, that they result for the most part from the adhesions of a plastic peritonitis, usually of mild chronic character. Membranes thus formed are usually thinner and less vascular than those which date from the fetal period, and, as remarked by Lardenois (*Jour de Chirurgie,* 1913), do not resemble embryonal peritoneum. The writer has experimentally produced such pericolic membranes in rabbits by inducing a low-grade plastic peritonitis. The mild adhesive peritonitis occurred as the result of the plugging of the large intestine with its own contents. Extreme coprostasis was induced by intermittent ligation of the anus and administration of morphine over a period varying from two weeks to three months. Care was taken not to confuse the normal, long-flowing mesenteries of the rabbits with membraniform adhesions. As in other quadrupeds, the mesenteries of the rabbit are luxuriantly developed, a single mesentery in some cases enveloping and supporting several intestines. The membranous adhesions which were attributed to the artificially induced plastic peritonitis occurred in cases in which the intestines were held together over extensive areas by plastic lymph. The membranes were brought to view by separating the intestine at the site of imperfectly organized adhesions. From these observations it appears that extensive membraniform adhesions may result from extreme coprostasis; and this is exactly as we should expect, for we know that incarceration of hernia and other forms of obstruction of the intestinal lumen, without visible lesion of the intestinal wall and without strangulation, lead to mild peritonitis, fusions, and membraniform adhesions. Such pericolic membranes are not different in histology and origin from similar membranes about the uterus and uterine adnexæ, all representing reactions of the peritoneum to irritation.

Of the types of pericolic membranes produced experimentally in rabbits, as mentioned above, one of the most common was that represented by a blanketing of the splenic angle with the fused anterior and posterior laminæ of the great omentum, suggesting an exaggeration of the phrenocolic ligament of Toldt. Hertzler and Cuneo attribute those vascular pericolic membranes with vessels coursing horizontal to each other in the line of the long axis of the ascending colon, or, transversely to it, to a pathological condition of the circulation. Cuneo believes that the blood-vessels produce the membrane. It is, however, no contradiction of the position of these writers to assume that the hyperemia or varicosity spoken of by them, as well as the insufficiency of the muscular coat of the intestine mentioned by Tuffier, represents reactions of the peritoneum to irritation. Lenormant and Oberlin (*Revue de Gynecologie et de Chirurgie Abdominale,* September, 1913) emphasize the importance of the relation of adhesion in plastic peritonitis to pericolic membranes. They make a distinction, however, between plastic membranous adhesions of adult life and those of embryonal origin. Adhesions in fetal life probably bear the same relation to pericolic membranes as do adhesions of the adult. It is probable that the true Jackson's membrane appearing upon the right half of the colon is usually an affair of fetal development, the result of accidents of cecal descent and torsion. On the other hand, membraniform adhesions of post-natal life, in the sense of Virchow, Gerster, and Pilcher, may appear anywhere upon the large intestine, the most frequent site probably being the splenic angle.

It seems likely that several factors enter into the causation of pericolic membranes; that the several factors are closely related; that fusion or adhesion as a result of reaction to irritation, hyperemia, or developmental accidents in the pre- or post-natal period may

lead to anomalous peritoneal formations; that in the fetal period of development abnormal fusions occur with notable frequency and with interesting constancy as to location and form; that pericolic membranes, unassociated with chronic or acute colitis or peritonitis, are of fetal origin; and, further, that to some extent the same pathological processes underlie pericolic-membrane formation at all ages from fetus to adult.

Although pericolic membranes, or pseudoperitoneums, present certain rather definite variations in structure, form, position, and relations, a correct classification of these formations can hardly be made with our present insufficient and disconnected data relating to their nature and origin. For the convenience of study, however, these anomalous membranes may be divided into three groups, as follows:

Group 1.—Those peritoneal sheets or ribbons, which, because of their position and attachments, seem to represent exaggerations of normal or anatomic fusions, folds, or ligaments, for example:

a. Those representing in an exaggerated or modified form: The phrenocolic ligament of Toldt, the costocolic ligament, the hepatocolic ligament, etc.

b. Those representing in an enlarged or modified form: The bloodless fold of Treves.

c. Those representing in amplified form: The fold of Reid (the genitomesenteric fold or Lane's band, which I think is the same thing).

d. Those representing in amplified form: The crescentic upfoldings of serosa which divide the space lateral to the ascending and descending colon into peritoneal fossæ.

e. Those representing in amplified form: The attachments of the great omentum to the colon, occasionally resulting in fusion of the omental serosa to the peritoneum of the colon throughout its length.

Group 2.—Pericolic membranes which correspond in form and position to anomalous peritoneal fusions or folds which appear frequently in the fetus, but which are not constant and therefore not anatomic, as:

a. The parietocolic folds of Jonnesco.

b. The several so-called root-folds.

c. Nondescript peritoneal folds about the appendix and caput coli.

d. Adhesions of the peritoneum of the colon to the upfolding peritoneum of its own mesentery. (Robinson and Reid.)

e. A blanketing of the colon by the fused anterior and posterior peritoneal laminæ of the great omentum. The fat of the omentum is replaced by an abundance of young parallel coursing blood-vessels, which vessels are disposed, as a rule, transversely to the colon.

Group 3.—Pericolic membranes occurring anywhere upon the large intestine, frequently over the splenic angle and descending colon, which represent reaction to irritation of inflammatory or other character, as—

a. Membraniform adhesions occurring about the colon the result of mild plastic or adhesive peritonitis, often secondary to chronic colitis. (Virchow and Gerster.) These are similar to the pseudoperitoneums seen occasionally upon the broad ligament, the uterine fundus, and the uterine adnexæ.

b. Changes in the peritoneum of the colon, due to the pathological condition of the circulation, as hyperemia or varicosities. (Hertzler and Cuneo.)

All of the varieties of pericolic membranes enumerated above have been observed in the fetus, and whatever the fundamental processes of the pathological conditions that cause these fusions and adhesions may be, it seems reasonably certain, that those processes are often active in fetal life and that all forms of pericolic

membranes, with the exception of those embraced in Group 3, can be traced to fetal fusion.

The failure to observe membranous pericolitis in young children has been interpreted, with some reason, as evidence showing that such membranes are not associated in any way with fetal peritoneal anomalies, but, on the other hand, must be consequent upon certain causative agencies, which, as a rule, do not become operative before maturity.

With a view of considering the collective experience of men who have large opportunities for observation in this field, and thus perhaps to shed some light upon the question of the incidence of pseudoperitoneums in children, as well as upon the subject of their nature and origin, communications of inquiry were addressed to representative teachers of surgery in this and other countries. Upon examination of the replies it appears that pericolic membranes have been observed during abdominal operations upon children by nearly one-half of the surgeons addressed. All of the following have found anomalous pericolic membranes in children of 12 years of age or younger:

Roux, of Lausanne; Stiles, of Edinburgh; Czerny, of Heidelberg; Albrecht, of Vienna; Lane, of London.

And the following Americans: Willy Meyer, C. H. Mayo, M. L. Harris, Flint, Binnie, McArthur, Gerster, Greensfelder, Hill, Summers, Knott, Haines, Coffey, Connell, Porter, Eisendrath, Hertzler, Finney, Sherman, Seelig, Collins, Cheever, Freeman.

Albrecht in 500 autopsies on children under 6 years of age found anomalous pericolic membranes in 125. Often in young persons Professor Roux has observed a veil of new and almost free peritoneum containing young blood-vessels disposed transversely. The symptoms in these children were like those of chronic appendicitis. Roux observed these membranes at various

levels upon the ascending colon. Stiles, of Edinburgh, has observed pericolic membranes in children from a few years of age upwards. Czerny has frequently seen anomalous peritoneal membranes upon the colon in children. M. L. Harris observed a typical Jackson's parietocolic membrane in a boy of twelve. Van Buren Knott reports the cases of two infants one year old operated on for intussusception who had well-developed Jackson membranes. Verhoogen, of Brussels, speaks of the possibility of his having overlooked pericolic membranes in childhood, for the reason that his attention has never been directed to them. Among those who had not encountered anomalous pericolic membranes in children, three remarked that they were doubtless present in childhood; but, since it is the infective accidents of later life which bring them into the clinical field, they escape observation in children. Flint observed the membrane in children eight, ten, twelve, and sixteen years of age, and believes that they occur in the same incidence in children as in adults, the reason that they are not more frequently seen being due to the fact that we operate upon adults ten times more frequently than upon children.

Samuel Harvey, Instructor in Pathology in Columbia University, did 108 autopsies on infants from one to two years of age. In 16.6 per cent of the cases he found membranes of the parietocolic type. In 13.9 per cent of the cases he found a membrane covering the caput coli. Lane's band, or Reid's genitomesenteric fold, was found in 9.3 per cent of cases. Flint says that the work of Harvey supplies the missing link between the fetal structures and the adult veils, and confirms absolutely their congenital origin; and he adds that he has no reason to change his views as expressed in his paper published in the *Johns Hopkins Bulletin* for October, 1912, in which he stated his conclusion that pericolic membranes of adults are in-

stances where the secondary fusions of the peritoneum in embryonic life are more extensive than we are usually accustomed to consider the norm. Many of us will agree with Flint, if, in stating this view, he excepts those membraniform adhesions and fusions of plastic peritonitis which certainly develop in adult life, but which, after all, should be classed elsewhere.

C. H. Mayo, Harold Stiles, Sir William Bennett, and others express the belief that membranous pericolitis must be of rather infrequent occurrence in children; and this view is borne out by the experience of many surgeons who, despite very great opportunities for observations, have not seen a case of Jackson's membrane in a child. Among these are—

Von Eiselsberg, Maylard, Handley, D'Arcy Power, Pierce Gould, Kellock, Wilms, Verhoogen, Pilcher, Crile, Kanavel, A. T. Mann, Law, F. H. Martin, J. B. Murphy, Littig, Grant Moore, and Jackson.

It should be borne in mind, however, that these men are surgeons, and therefore would only observe such pericolic membranes as are associated with clinical manifestations.

A few of those who do not approve the hypothesis of embryonal origin of any variety of pericolic membrane report the frequent finding of Jackson's anomalous membrane in association with cecum mobile. However, this combination in itself furnishes evidence suggesting a fetal origin; for in the mobile cecum, swinging as it does through an abnormally large arc, we have an instance in which a secondary fusion between the cecum and the mural peritoneum may be drawn out readily in a degree and form quite beyond the norm.

It is the almost constant observation that when, during operation for removal of the appendix vermiformis, the uninflamed appendix is found buried under

a pericolic membrane, such a veiled and pocketed appendix is almost always of fetal type, that is, long and very slender. Such membrane-covered appendices are often not more than two millimetres in diameter, and are rarely of a diameter exceeding three millimetres; and they show many evidences of interrupted development.

FOR DISCUSSION, SEE PAGE 179

THE SIGNIFICANCE OF INTRA-ABDOMINAL "BANDS," FOLDS," AND "VEILS"

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[PRESENTED BY INVITATION]

INTRODUCTORY REMARKS

When I first began, about eight years ago, to study the various bands, folds, membranes, and other adventitious structures found within the abdomen, and to consider the relationship of these to chronic intestinal stasis, little heed was paid by the general body of the medical profession to this field of investigation in which Sir Arbuthnot Lane, of London, is unquestionably the pioneer. It is, therefore, with keen pleasure that I have watched the awakening of interest, of which this symposium is, in a way, the culmination. A few years ago Lane's statements with reference to the existence of these newly formed structures, to which he gave the name *evolutionary bands,* were received with incredulity; and his conclusions with regard to relationship between these tissues and chronic intestinal stasis were met with skepticism. Now, however, as he has told us, so many "bands," "folds," "veils," and "membranes" are being found within the abdominal cavity that some maintain that they cannot possibly be the cause of chronic intestinal stasis.

While doubt concerning their existence has been practically eliminated by repeated verification on the part of many observers, the origin and effect of these structures continue to call forth differences of opinion.

There has become manifest, too, in the development of the entire subject, that unfortunate tendency which so often characterizes scientific work, to label things with the names of those who first specifically described them. And so we have the "bloodless fold of Treves," the "parietocolic peritoneal fold of Jonnesco and Juvara," the "genitomesenteric fold of Reid," the "ileopelvic band," and other "bands" of Lane, and the newly-formed veil of peritoneum, *Jackson's membrane,* the last being the special subject for discussion on this occasion.

It seems to have escaped the attention of many investigators that Sir Arbuthnot Lane, in 1901,[1] first called attention to a "large vascular peritoneal apron," having an attachment to the abdominal wall outside the cecum and colon, and extending inwards over it and being firmly attached to its surface." "Its object," he said, "is clearly to limit the range of movement of the cecum, and to control its distention." This vascular peritoneal apron, according to Lane, is continuous with bands deep in the loin. These bands of peritoneum, which are sometimes spoken of as adhesions, are attached posteriorly deeply in the loin, and are often very strong, giving rise to obstruction of the upper part of the ascending colon, and producing distention and hypertrophy of the cecum. Aside from the inflammatory cases, where the adhesions are the sequel of peritonitic or appendiceal processes, other cases were explained by him as having apparently developed in consequence of an habitual distention of the cecum, as the result of constipation. This state of affairs, he maintained, is very common, but not sufficiently recognized.

In 1903 Lane[2] again called attention to the subject of this interference with the drainage of the intestinal canal, and again emphasized the vital importance of its recognition. He contended that the free passage of the intestinal contents through the hepatic flexure is interfered with by the development of a mesentery which is attached to the outer surface of the cecum, ascending colon, and the hepatic flexure. He presented a diagram showing the "bridles" formed by the shortening up of the longitudinal fibers on the front of the ascending colon, stating that these "bridles" are so marked in some cases as to require to be partly divided, in order to restore the colon and cecum to their normal position and their capacity for the efficient performance of their function.

When I first began to study these conditions with Lane, this peritoneal apron to which your distinguished president subsequently gave the name "pericolic membrane," as well as the various other folds and bands which are commonly found in chronic intestinal stasis, were demonstrated to me by him numbers of times at Guy's Hospital. They were not given specific names, and were not considered by him as separate entities, each with its own particular sphere of activity in the production of abnormal conditions within the abdomen, and each invested with its own particular set of symptoms. He rather considered them as parts of the great whole,—the causation and symptomatology of chronic intestinal stasis.

Lane's dominant idea in studying the cases which have come to be known under the diagnostic term of "chronic intestinal stasis," was to work back from the widely divergent manifestations which we now consider to be the symptom-complex of stasis, to the underlying cause or causes. At the same time, naturally, he was working in the opposite direction, from the abnormal intra-abdominal manifestations which have

come, carelessly and without his approval, to be known as "Lane's bands," and "Lane's kinks," to the possible pathological effects produced by these. Coincidentally, of course, he studied the pathogenesis of these intra-abdominal tissue formations.

Thus Lane, recognizing the existence of these pathological structures within the abdomen, studying their pathogenesis, tracing their relationship to a wide variety of pathological manifestations, and developing a system of preventive and correctional measures, has played the rôle of the true pioneer, pointing the way to a new field for exploration, a new territory for useful endeavor. Lane, in other words, has stood on the mountain-top, so to speak, and has swept his eye completely around the horizon.

Having worked with him on my various visits to London for several years, having studied his cases before, during, and after operation, having repeatedly seen his hypothesis with reference to the existence of these causative factors in the production of chronic intestinal stasis verified upon the operating-table, and having substantiated his findings in a large series of my own cases, I have tried to follow his example, and to sweep my own mental vision around the horizon rather than to fix my gaze upon this, that, or the other particular point.

Therefore, with no possible desire to detract in the slightest degree from the brilliant work of your distinguished president, of Jonnesco and Juvara, of Douglas Reid, of Treves, and of the many others who, latterly, have contributed to the investigation of this interesting and important subject, I wish to emphasize what seems to me to be the logical point of view, that of considering all these intra-abdominal abnormal structures, whatever their origin and situation, as parts of the great whole, as possible factors in disturbing

the body-drainage in the production of chronic intestinal stasis.

For purposes of convenience of comparison I append brief descriptions of the various structures which come into consideration in connection with the bands described by Lane:

1. *The Bloodless Fold of Treves.*[3]—This fold is described by Reid[4] as follows: "Passing from the mesenteric border of the appendicular cecum and from the nonmesenteric border of the last part of the ileum to the posterior and lateral abdominal wall is a sheet of the peritoneum. This forms the anterior wall of a fossa completely closed save below. Its orifice is bounded in front by the free lower edge of the peritoneal sheet, medially by the ileum and the adhesions which bind this down, and posteriorly and laterally by the parietal peritoneum."

The fold of Treves passes over the caput coli and appendix, and, as Eastman[5] has pointed out, may adhere to the abdominal wall and resist for a time the completion of cecal torsion. Eastman suggests that the lower inner border of the bloodless fold of Treves may resist the drawing upward and inward of the last part of the ileum, thus preparing conditions favorable to the formation of Lane's kink, or actually causing it.

The Parietocolic Peritoneal Fold of Jonnesco and Juvara.[6]—In the majority of the cases this fetal fold arises from the peritoneum at the left or inner side of the ascending colon, passing over the anterior aspect of the ascending colon in an upward slanting direction. It is attached to the parietal peritoneum at the right of the ascending colon. It may adhere to the anterior and lateral aspects of the colon.

Practically the same relations as are presented by the parietocolic fold or Jackson's membrane, when

found in the adult, have been attributed by Reid[6] to the above-described fetal fold.

Several succeeding stages of the formation of Jonnesco's fold were found by Eastman,[6] indicating that adhesions may form between the serosa of the cecum and the mural serosa while the cecum is still subhepatic. The subsequent descent, with torsion or rolling inward upon the long axis, draws the parietal serosa over in front of the ascending colon in a slanting or spiral direction, like a ribbon wound spirally around a rolling cylinder.

"Jackson's Membrane."—Jackson[7] describes this structure as follows: "From a point just at the hepatic flexure to three inches above the caput coli there spreads from the parietal margin over the external lateral margin to the internal longitudinal muscle band, a thin vascular veil, in which long, straight, unbranching blood vessels course, most of which are parallel with each other and take a slightly spiral direction over the colon from the upper peritoneal attachment to the inner lower portion of the gut ending just above the caput. The appendix is not implicated in any way. Coursing with the blood-vessels are numbers of shining narrow bands of connective tissue, which gradually broaden as they go and end in a slight fan-shaped attachment at various points on the anterior and inner surfaces of the colon. At these points of attachment the gut is held in rigid plication. At the beginning of the hepatic flexure the drawn membrane particularly angulates the contained colon."

The Genitomesenteric Fold of Douglas Reid.[8]—This fold of peritoneum, which is said to be rather common, passes from the mesentery of the terminal segment of the ileum down into the pelvis, being attached in the fetus to the genital gland below. It has, therefore, a secondary connection with the ileum and, through the

peritoneum of the meso-appendix, with the appendix itself, under which it passes. This connection is perhaps responsible for the frequent association of appendicitis and oophoritis. Adhesions of the colon to the peritoneum of its own mesentery were also pointed out by Reid.

This genitomesenteric fold is found in a surprisingly large percentage of fetuses after the seventh month, or even after birth at term, and Eastman[9] has found it in the adult.

"Attic Adhesions" (Morris).[10]—"Anatomists have noted that web-like adhesions were found in the bile-tract region so frequently that they seemed to be almost a normal characteristic of the region. We used to feel the same way about adhesions in the cecal region. Byron Robinson called attention to the fact that adhesions were found in the bile-tract region more frequently than elsewhere in the peritoneal cavity excepting in the pelvis in women; and that the cecal region stood third in order of abundance of adhesions." Peritoneal adhesions, according to Morris, are found to some degree in almost every adult on post-mortem examination. Those in the upper part of the abdomen he has aptly called "attic adhesions."

Origin of Intra-abdominal "Bands," "Folds," and "Veils."—Three chief theories concerning the pathogenesis of the intra-abdominal structures under discussion have engaged the attention of the different investigators: (1) mechanical or evolutionary (Lane), (2) embryonic or congenital, (3) inflammatory and infectious. In addition to these, the combined congenital and acquired factors have been suggested as playing a part in the development of these structures as they are found in the adult.

Embryonic or Congenital Theory.—In an early stage of fetal life the entire abdomen is filled with bands,

resembling the conditions seen after an attack of peritonitis.[11] Toward the conclusion of fetal life these bands disappear, and the adhesions melt away, although some may persist, as certain investigators have maintained. In order to determine how frequently such bands may persist and become factors in the production of chronic intestinal stasis or other abnormal condition involving the abdominal viscera, it will be necessary to examine carefully a large number of young infants as well as fetuses at various stages of development.

Chapple, in a recent personal communication, reports having dissected twenty-eight fetuses without finding any bands of the type associated with "Lane's kinks" and chronic intestinal stasis. Professor Keith, of the Royal College of Surgeons, London, examined in my presence last fall, fifteen fetuses, in three of which he thought there was a rudimentary band that might be a factor in producing stasis. I must say that I was not convinced of this, as the bands were not at all similar to those found in the adult static individual. Eastman and other investigators have found a larger proportion of these pre-natal structures which have been thought to persist, in some cases, in the adult, giving rise to some of the disturbances in which the gastrointestinal tract is concerned. It is not within the scope of this paper to discuss the weight of evidence concerning the theories proposed to account for the existence, in the fetus, of these abnormal structures. More study is necessary before a conclusion can be reached.

Inflammatory and Infectious Theory.—The inflammatory and infectious bands may be classed as (1) endogenic, resulting from peritoneal reaction from infection within the colon, and (2) exogenic, resulting from infections transmitted from some inflamed organ

in the immediate vicinity. Prolonged or repeated mild infections of the peritoneal covering of the cecum and appendix, transmitted through the intestinal wall, are considered by Pilcher[12] as a causative factor in the production of these membranous bands. Gerster[13] assumes a reaction of the peritoneum to infection and chronic inflammation of the colon in the form of "characteristic vascularized transparent membranes (pseudoperitoneum), which take their origin along the external lateral aspects of the cecum, ascending colon, and hepatic flexure on one side, and the sigmoid flexure, descending colon, and splenic flexure on the other."

Inflammation, undoubtedly, plays a part in the formation of some forms of the membranous structures found within the abdominal cavity, and hence in the production of intestinal obstruction of one kind or another. It seems to me, however, that all these formations are to be distinguished from those which Lane describes as being of mechanical or evolutionary origin. For, as Lane has shown, these acquired bands, which he calls evolutionary, commence at a point most distant from the intestine, gradually approaching and securing the bowel. Inflammatory bands, on the other hand, commence at the point of attachment, progressing away from this.

Mechanical or Evolutionary Theory.—Of the various theories proposed to account for the structures under discussion, that of Lane is, to my mind, the most plausible, accounting, as it does, for a much larger proportion of the cases than does either the embryonic or the inflammatory theory.

In a recent communication,[14] in discussing the influence of alimentary toxemia upon the bony structures, Lane laid down the following three laws as governing the changes which take place in consequence

of the altered mechanical relationship of the individual to his surroundings:

(1) "That pressure produces changes in the structure and form of the bones and in the form and function of existing joints, while it determines the formation of new joints."

(2) "That strain produces change in the form of the bones, and in the form and function of existing joints, and also produces new joints."

(3) "That when, apart from the exercise of pressure or strain, it is important, from the altered mechanical relationship of the individual to his surroundings, that a mechanism should be modified or an entirely new one developed, such a change takes place."

These same mechanical principles, according to Lane, govern the soft parts of the body and modify their structure. Following this line of reasoning, he has applied these laws to the abdominal cavity, and in this manner has accounted for the development of the various adventitious structures which he associates with chronic intestinal stasis in the majority of cases.

The membranous adhesions between the outer wall of the colon and the abdominal parietes, as well as in other segments of the intestinal tract, which were noted by Lane in cases of chronic intestinal stasis, were interpreted by him as newly developed accessory ligaments, serving to counteract the tendency to enteroptosis incident to the disturbed mechanical relations of the bowel consequent upon the assumption by man of the upright position. In other words, the origin of these bands about the ascending colon, hepatic and splenic flexures, sigmoid, and other portions of the gastro-intestinal tract is referred by him to the mechanical factors of chronic constipation and the upright posture. He holds that nature attempts to relieve the strain of the persistent enteroptosis, and

that the dragging of the displaced bowel is offset, as it were, through hypertrophy of its membranous supports, "the crystallization of resistance," "the crystallization of lines of force,"—a physiological response to a mechanical demand, which conforms to the three laws as stated by him.

The mechanical relations of the large bowel are impaired primarily by the overloading of the large gut and of that portion which serves more especially as the cesspool of the gastro-intestinal tract, the cecum, and the ascending colon. The excessive distention of this portion of the tract results very largely from the erect or sedentary posture of civilized man. While the cecum and ascending colon have great difficulty in passing on their contents in the erect posture, the evacuation of the cesspool is very much facilitated by the assumption of the supine position. Through the changed mechanical relations of the bowel, intestinal surfaces not normally in contact come into apposition, with the result that more adhesions form and interfere further with the passage of material through the lumen already constricted at the flexures. Obstruction of the appendix due to its acquired abnormal fixation, is a very common concomitant of the alteration in the form and mechanical relations of the cecum, and Lane directs attention to the relationship of so-called "appendicitis" to the overloading of the cesspool or cecum.

Lane has pointed out that obstruction in the drainage scheme may occur, in the first place, at points of normal fixation; that, in the second place, the intestine may become kinked by acquired bands, etc., "which, formed in the first instance by nature, with the object of facilitating drainage, may have later come to cause very material obstruction." He has also directed attention to the various sites in the length of the alimentary tract at which this obstruction may occur, the

points of predilection being: (1) in the third part of the duodenum, at the commencement of the jejunum; (2) at different points along the terminal coil of the ileum; (3) in the ileocecal region, including the appendix; (4) in the region of the hepatic flexure and the first part of the transverse colon; (5) at the splenic flexure; (6) at the sigmoid loop; (7) in the rectum.

These various changes involving the large bowel are admirably shown in Fig. 1 (Fig. 43, *op. cit.*, Lane, p. 84).[15]

I may say, in this connection, that in my own cases I have repeatedly verified many of Lane's findings, as I shall show upon the screen. It is often my practice, when operating upon patients whose condition has been shown, by the various diagnostic methods at our command, to be chronic intestinal stasis, to have an artist and a stenographer in the operating-room. The artist, who is skilled in the recognition of anatomical conditions and the portrayal of these upon paper, sketches the conditions found in each case, taking accurate notes for purposes of identification. The stenographer notes the description of the condition and its treatment, as dictated by the operator. The finished drawings are then checked up by those who saw the operation. The pictures which I shall present, as verification upon the operating-table of conditions portrayed by Lane in the diagrammatic sketches which I shall throw upon the screen, were made in the manner described. They have been selected from a large number merely as evidence of the existence of these bands, and as seeming to corroborate Lane's view with regard to the evolutionary nature of these structures.

"Jackson's Membrane" and Ileal Kink. Fig. 2 (Fig. 32, *op. cit.*, Lane,[16] p. 25).—The tendency to displacement in the dilated cecum in a downward and inward direction is regarded by Lane as acting along

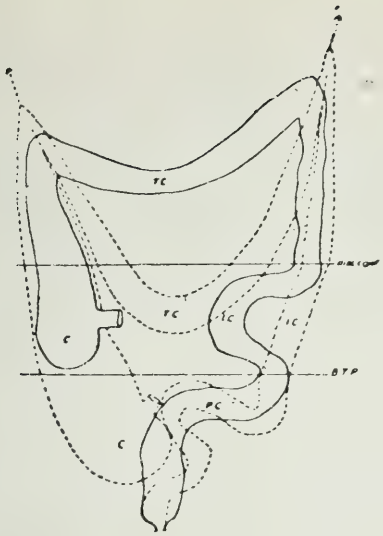


Fig. 1. Represents the several variations from the normal which the large bowel undergoes. The normal condition is shown as a firm outline, and the altered condition as a dotted outline. Note the prolapse of the cecum and transverse colon, the telescoping of the iliac colon and the elongation of the pelvic colon. BTP indicates the brim of the true pelvis. The hepatic and splenic flexures are drawn up and kinked by the development of acquired resistances shown by arrows.

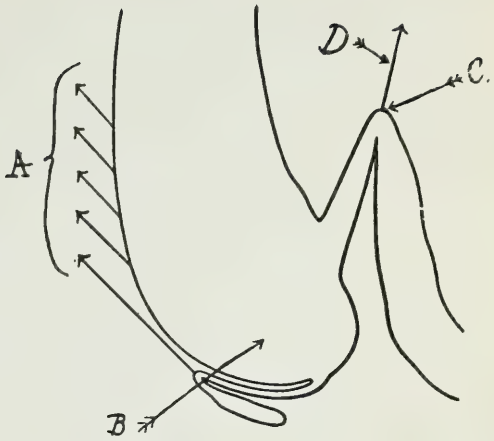


Fig. 2. A. Jackson's membrane, the lower part of which involves the appendix. B. The distended loaded cecum. C. Ileum kinked by band D.

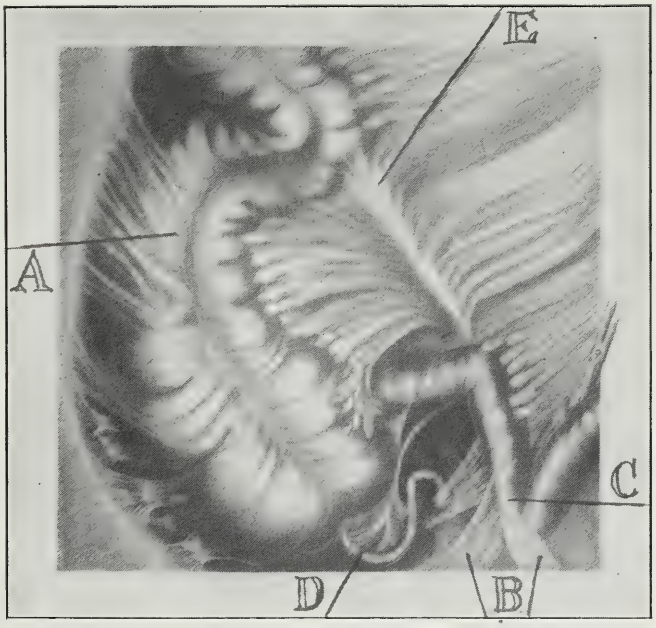


Fig. 2a. A. Jackson's membrane. B. Multiple Lane's bands. C. Lane's kink. D. Appendix caught in bands. E. Thickened part of mesentery forming a strong band.

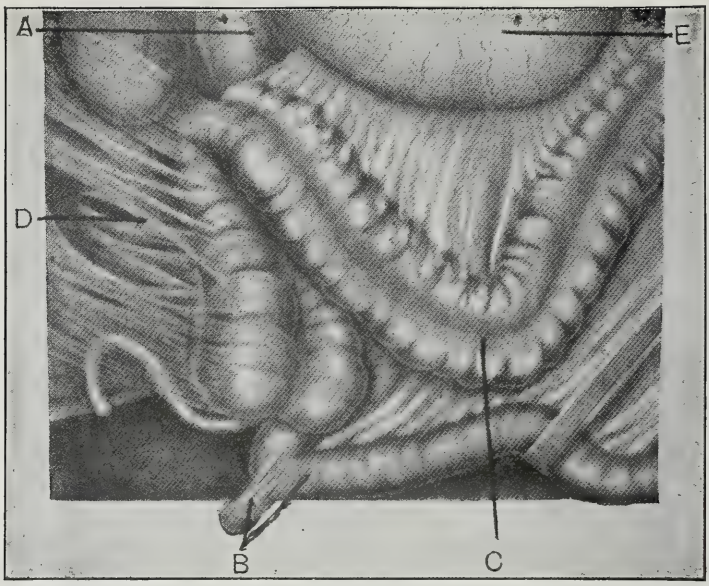
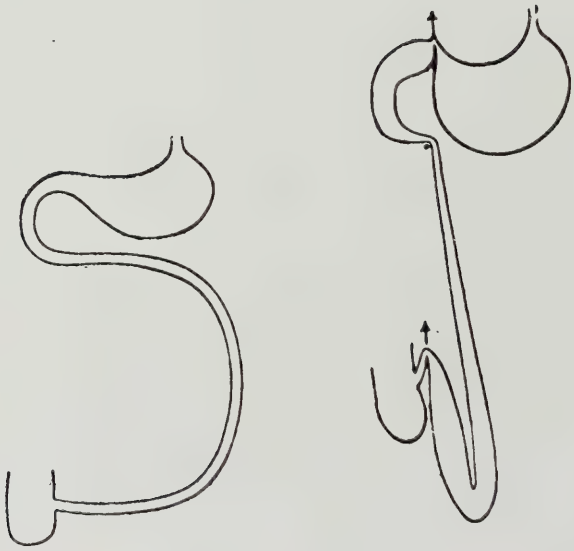


Fig. 2b. (From B. M. J., Nov. 1, 1913.) A. Dilated duodenum. Angulated duodenojejunal junction. B. Lane's band and kink. C. Prolapsed transverse colon. D. Jackson's membrane. E. Gastric dilatation.



Figs. 3 and 4. 1. Normal condition of stomach, small intestine and cecum. 2. The several changes which result from ileal obstruction.

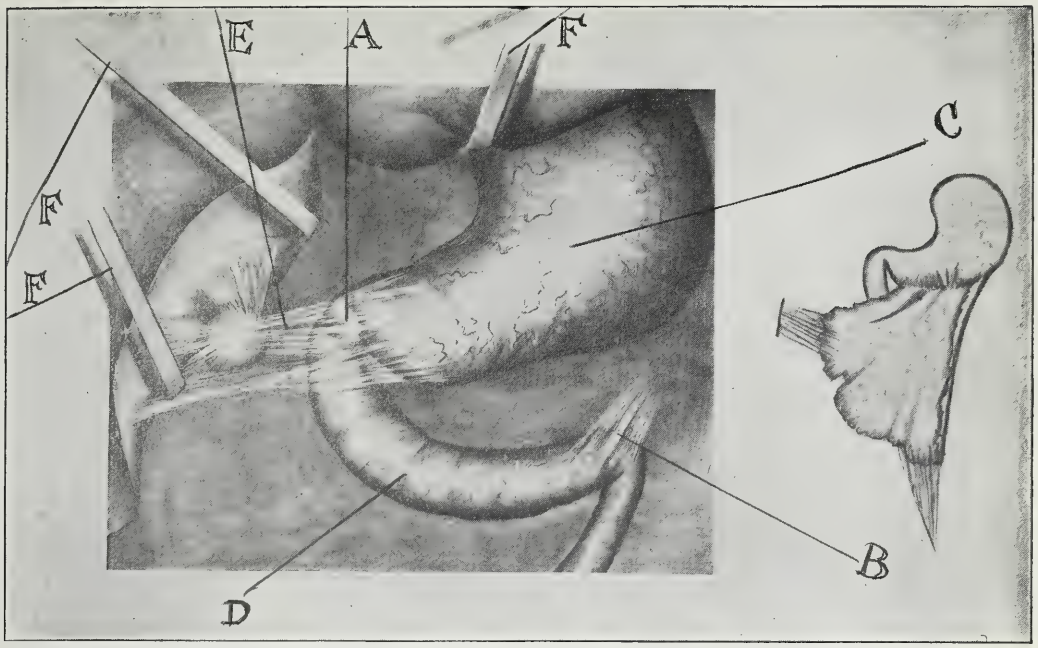


Fig. 4a. Kinking at pyloric outlet. B. Duodenojejunal kink. C. Dilated stomach. E. Bands to gall-bladder. F. Retractors holding up liver.

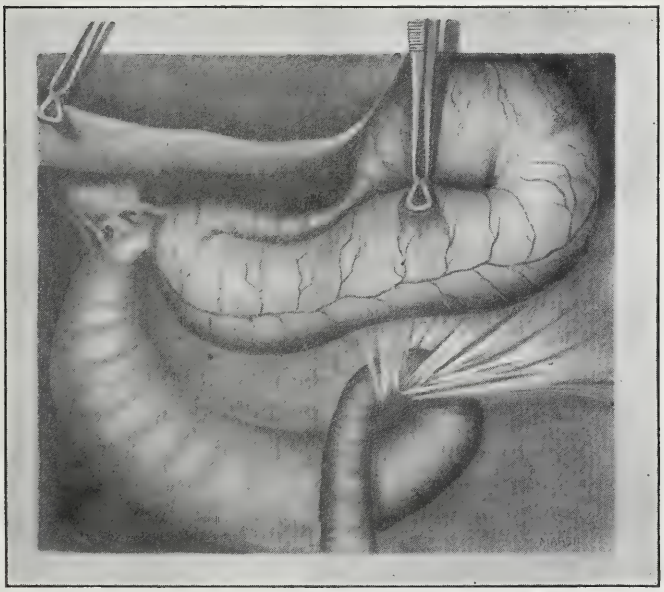


Fig. 4b. Another illustration of practically the same conditions shown in Fig 4a, only more marked and loop of jejunum thrown to the right.

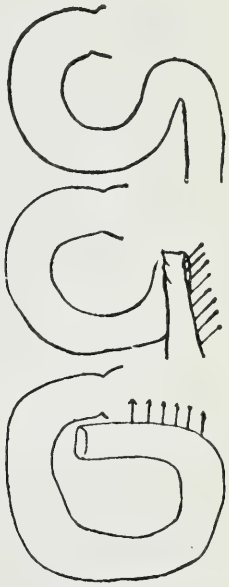


Fig. 5. Normal curve of duodenojejunal junction.

Fig. 6. Angulation of duodenojejunal junction produced by bands.

Fig. 7. Nature's efforts, by the formation of bands, to prevent angulation.

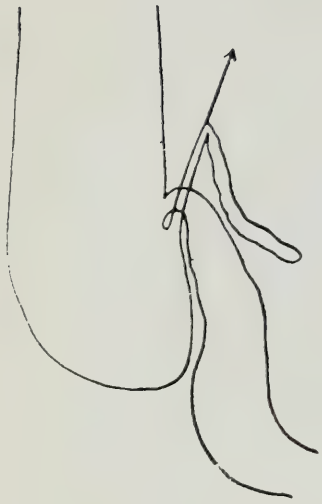


Fig. 8. "Appendiceal Tie," with ileum kinked above by the appendix, and distended below.

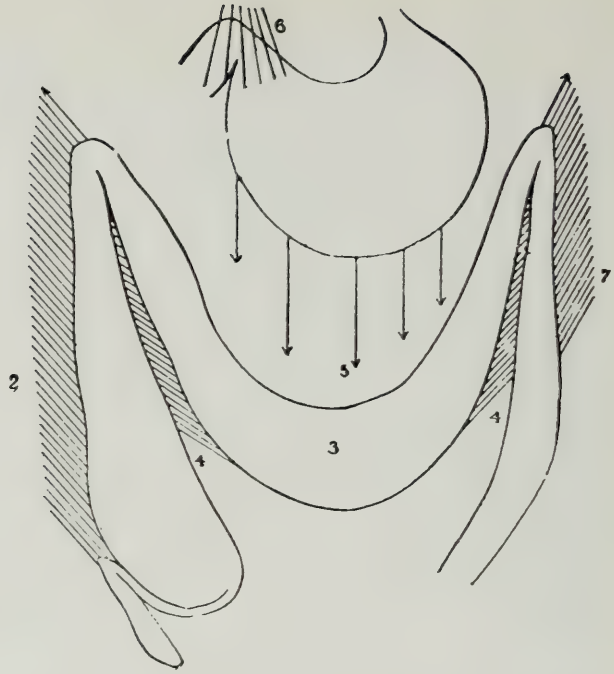


Fig. 9. 1. Represents the prolapsed cecum. 2 and 7. The crystallized resistances which tend to oppose the downward displacement of the large bowel and sustain some of the weight of the transverse colon transmitted through the crystallized resistances (4). 3. The transverse colon. 5. Portion of the weight of the transverse colon transmitted through the great omentum to the convexity of the stomach. 6. The acquired ligament that secures the duodenum and pylorus to the under-surface of the liver and gall-bladder.

Fig. 8a. A. Appendix caught up by band. B. Retractor holding up ileum. When ileum is allowed to drop over appendix, the condition shown in Fig. 8 results.

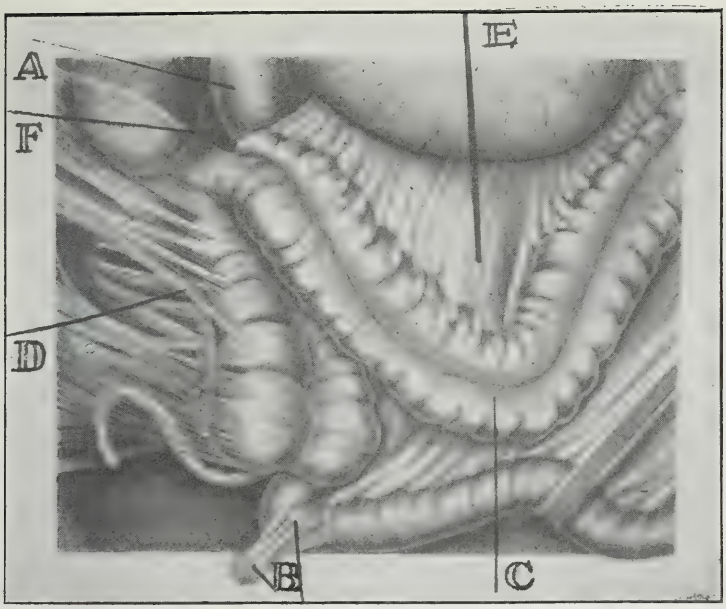


Fig. 9a. (From B. M. J., Nov. 1, 1913.) Also Fig. 2b. A. Dilated duodenum. Angulated duodenojejunal junction. B. Lane's band and kink. C. Prolapsed transverse colon. D. Jackson's membrane. E. "Portion of the weight of the transverse colon transmitted through the great momentum to the convexity of the stomach." F. Acquired ligament securing duodenum and pylorus to under surface of liver and gall-bladder.

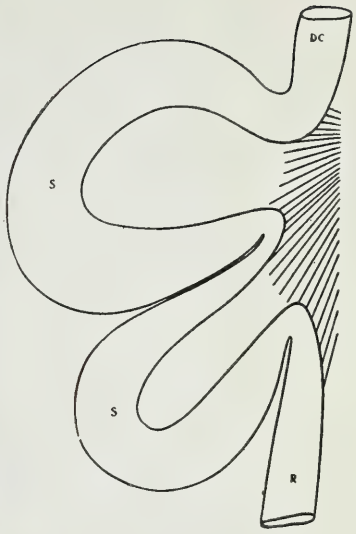


Fig. 10. Fixation and obstruction of sigmoid.

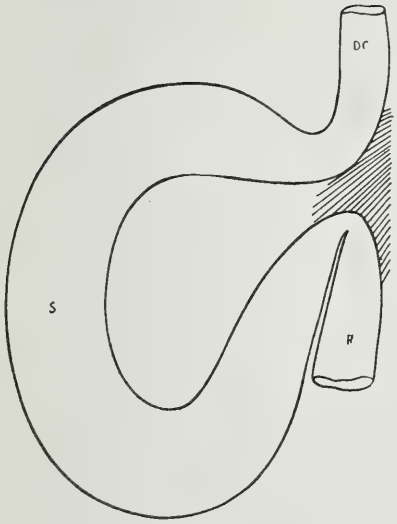


Fig. 11. Diverticulitis of sigmoid produced by approximation of two ends of loop of sigmoid.

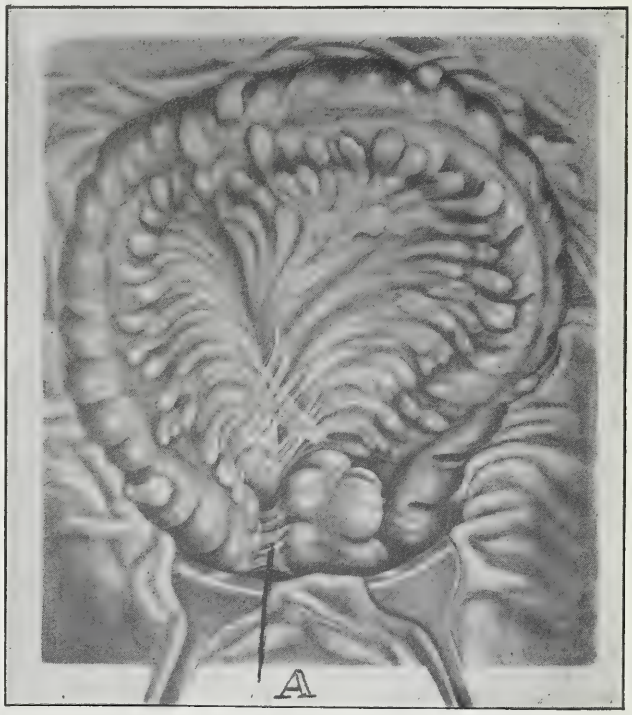


Fig. 11a. (From Medical Record, Sept. 27, 1913.) A. Band holding in approximation two ends of sigmoid loop, causing a diverticulum.

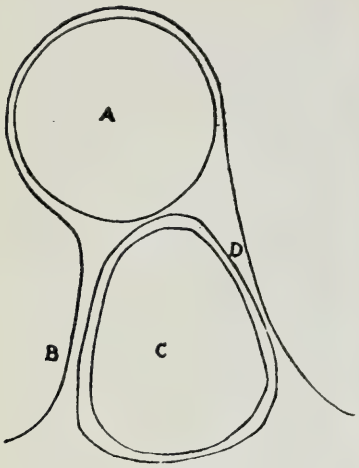


Fig. 12. A. Represents the colon in transverse section. B. The original mesentery. C. The cystic ovary lying free in a cavity. D. The remains of the acquired adhesions.

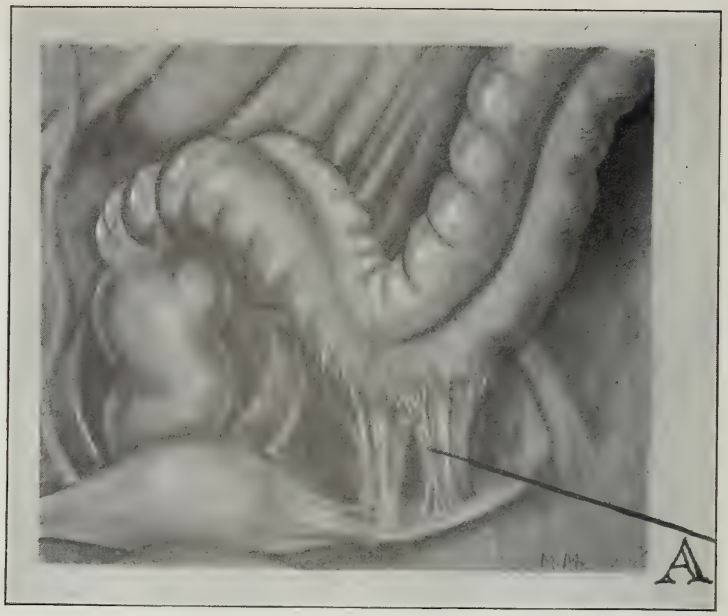


Fig. 12a. (From Medical Record, Sept. 27, 1913.) A. Band in which stump of tube and ovary is caught.

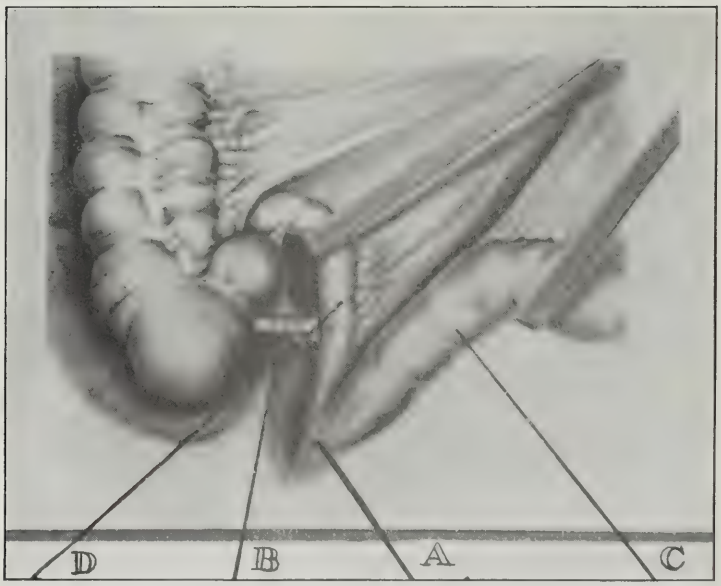


Fig 13. A. Lane's band and kink. B. Appendix caught behind. C. Dilated and thickened ileum. D. Collapsed and thinned ileum.

the resultant of a parallelogram of forces. In order to oppose this tendency, resistances develop which correspond in position and action to the sides of the parallelogram. These lines of resistance, Lane maintains, "are crystallized first as bands, and later as distinct membranes, which, as the outer limb of the parallelogram, connect the peritoneum lining the abdominal wall to the outer surface of the colon in its immediate vicinity. A larger area of abdominal peritoneum and of colon is gradually involved, and later still, as is the case in all resistances which are crystallized in peritoneum, the bands are replaced by a layer of peritoneum, which clings and tends to support the colon from the outside. As these bands and membranes develop, blood-vessels form in them which later may become sufficiently large to require to be ligated if divided. This particular resistance, when fully developed into a membrane, has recently been termed 'Jackson's membrane.' "

The distended loaded bowel is here shown dragging upon the resistances evolved to oppose its downward displacement into the true pelvis. The resistances external to the cecum are indicated by an arrow showing the general direction of the strain exerted by bands, adhesions, and omental structures. The lowest of these secures the appendix, which is kinked. The effects exerted upon the ileum at its point of fixation by the weight of the loaded cecum on the one side of it and by that of the obstructed loop of the ileum on its proximal aspect, are shown.

Fig. 2-a, from my own series, represents Jackson's membrane and the ileal kink. Fig. 2-b, which will be presented again,[17] shows the appendix caught back in the parietocolic membrane, in a fashion similar to that portrayed diagrammatically in Fig. 2.

The "crystallized resistances of peritoneum" are not limited to the cecum, but may extend up along the

outer aspect of the ascending colon, even to the hepatic flexure, as we have already seen.

Ileal Bands.—The inner limb of this parallelogram of forces is represented, according to Lane, at an early period by opaque streaks on the under surface of the mesentery, attaching the last few inches of the ileum, and commencing at its base at a point most distant from the bowel. The reason for this development he considers obvious: "Besides retaining the end of the ileum in position, the mesentery assists through the medium of the termination of the small intestine in holding up the cecum and in tending to oppose its downward displacement." "In its earliest development," he says, "as is the case with most conditions which evolve during the lifetime of an individual, because of a variation from the normal in the relationship to his environment, the effect of this acquired ligament or mesentery is useful and physiological, but later, when it kinks and obstructs the lumen of the ileum, it exerts a progressively deleterious effect on the well-being of the individual."

When this state of affairs develops, the inner limb of the parallelogram of forces, according to Lane, is represented by the portion of the ileum distal to the kink, "by the contracting acquired membrane which creeps around the circumference of the bowel, and by the posterior layer of the mesentery to which the membrane is attached. The upper layer of the mesentery presents no change, as abnormal strain is not exerted along it, but through the under layer only. In these circumstances the portion of the ileum between the cecum and the attachment of the acquired ligament is made to perform the function of a ligament very much more than is the case in the normal arrangement."

In Figs. 3 and 4 (Figs. 36 and 37, *op. cit.*, Lane, p. 30), respectively, are diagrammatically indicated:

(1) the normal condition of the stomach, small intestine, and cecum; (2) the several changes which result from ileal obstruction. The part of the ileum just beyond the kink in the ileocecal region becomes dilated and drags upon the duodenojejunal junction, at which point a crystallization of lines of force takes place, with resultant kinking. The duodenum, as a consequence of this obstruction to its drainage, becomes dilated, throwing an abnormal strain upon the lesser curvature in the neighborhood of the acquired suspensory ligament with which nature has endeavored to forestall trouble. This ligament becomes crystallized into a "resistance," and finally a kink takes place at the pyloric outlet. These conditions are indicated in Fig. 4-a and Fig. 4-b, of my series.

Duodenojejunal "Resistances" and Kink.—Figs. 5, 6, and 7 (Figs. 39, 40 and 41, *op. cit.,* Lane, p. 80) are further illustrations of possible duodenojejunal conditions. Fig. 5 shows the manner in which the duodenum usually terminates vertically at the root of the transverse mesocolon, where it is continued into the jejunum as a gentle curve. Fig. 6 represents the kinking and twisting at the duodenojejunal junction as the result of the strain exerted by the jejunum, and the consequent obstruction to the effluent from the duodenum. The resistances which, as peritoneal bands, are developed to oppose this drag upon the jejunum and the obstruction of the duodenal outlet, are represented by the arrows. These bands, it will be seen, run upwards from the commencement of the outer aspect of the jejunum to the peritoneum lining the adjacent abdominal wall. Fig. 7 represents nature's efforts to ward off angulation at the duodenojejunal juncture by the development of "resistances crystallized in membrane," fixing the jejunal loop so as to avoid kinking. Fig. 4 of my series shows the angula-

tion. I happen not to have in my series one to illustrate Fig. 7.

Lane has repeatedly emphasized the far-reaching effects of obstruction at the duodenojejunal junction. The distention of the duodenum, the dilatation of its lumen and hypertrophy of its wall, the infection of the contents with alien organisms, the lowering of the vitality of the gut by auto-intoxication, vascular enlargement, varying degrees of inflammation, ulceration, possible perforation, possible cicatrization, and stenosis of this portion of the bowel, are some of the phases of this condition. In consequence of infection of the duodenal contents, the pancreatic and common bile-ducts, which open into the duodenum, are infected, resulting, according to Lane, in more serious affections of the pancreas and the liver.

"Appendiceal Tie."—In Fig. 8 (Fig. 38, *op. cit.*, Lane, p. 31) is represented diagrammatically the mechanism of the "appendiceal tie," or the "resistance" when it forms part of the inner limb of the parallelogram of forces. The cecum and ileal loops are both here shown to be distended with fecal contents, and as having fallen into the true pelvis. The end of the ileum is here shown as it hangs over the fixed portion of the appendix, which is secured by its acquired membrane or crystallized resistance (indicated by the arrow). The lumen of the ileum is thus diminished by this fixed portion of the appendix. The greater the drop of the cecum, as it pivots on the fixed appendix, and the greater the dropping and distention of the ileum, the more complete does the obstruction by the appendiceal tie become. This, I think, is perfectly apparent.

In Fig. 8-a, from my series, the appendix is caught up in such manner that when the ileum is not held up, as it is shown in the picture, it drops over the appen-

dix, and becomes kinked in the manner shown in Fig. 8.

"Resistances" Which Involve Pancreas, Liver, Stomach, and Large Intestine.—Fig. 9 (Fig. 42, *op. cit.,* Lane, p. 35) represents the mechanism whereby the pancreas, liver, stomach and large intestine, as well as the small intestine, are involved in the development and crystallization of lines of resistance. In this diagram 1 represents the prolapsed cecum; 2 and 7, the crystallized resistances which tend to oppose the downward displacement of the large bowel and sustain some of the weight of the transverse colon transmitted through the crystallized resistances, represented by 4; 3, the transverse colon, prolapsed; 5, portion of the weight of the transverse colon transmitted through the great omentum to the convexity of the stomach; 6, the acquired ligament that secures the duodenum and pylorus to the under surface of the liver and gall-bladder.

Fig. 9-a, of my series, represents these conditions.

Fixation and Obstruction of Sigmoid.—Figs. 10 and 11 (Figs. 46 and 47, *op. cit.,* Lane, pp. 41 and 43, respectively) represent (1) fixation of the sigmoid loop, and (2) the production of volvulus by the approximation of the ends of the loop, by means of acquired adhesions.

Fig. 11-a, from my series, is an excellent illustration of the condition represented in Fig. 11.

The "Last Kink."—The "last kink," which affects the large bowel where it crosses the brim of the true pelvis on the left side, is represented in cross section by Fig. 12 (Fig. 48, *op. cit.,* Lane, p. 92). Lane attaches great importance to this obstruction, which develops early in life. The bands which cause this obstruction form on the outer surface of the mesentery

of this part of the large bowel, and are, in his opinion, like all other acquired mesenteries and adhesions, the crystallation of lines of resistance to downward displacement. The ovary and Fallopian tube are often involved in these adhesions.

Fig. 12-a, of my series (from the *Medical Record,* Sept. 27, 1913), shows adhesions between the stump of the ovary and tube and the sigmoid. The ovary had been caught in the adhesions, and had been removed before I saw the case. The adhesions re-formed about the stump, as shown in the picture.

Fig. 13 represents the findings in a case of marked chronic intestinal stasis, operated on by Sir Arbuthnot Lane, the writer assisting, at the New York Polyclinic Hospital, Nov. 28, 1913. It shows a small "Lane's band," an ileal kink, the appendix caught up behind, a dilated ileum with much thickened wall, and the collapsed part of the ileum with thinned wall. At operation a non-vascular band was found angulating the terminal ileum and binding it down deeply in the pelvis. General visceroptosis. Duodenum dilated, extending almost into the true pelvis. Marked secondary evidence of static condition in other organs. Note the absence of Jackson's membrane and other bands.

From this categorical summary of Lane's exposition of the changes which take place, within the abdomen, in consequence of the altered relations brought about by the assumption of the upright posture, it seems to me that he makes out a very good case for his mechanical or evolutionary theory of the origin of all the adventitious structures which, in his opinion, play so important a part in the production of chronic intestinal stasis. It may be said, however, that much work needs yet to be done before any theory can be firmly established. We are only beginning to explore the field which he has opened to us. The establish-

ment of the existence of these various structures takes us a long way. We have yet to determine more about their origin, and to follow the leads which he has given us with reference to the far-reaching effects of the conditions brought about by the development of these structures. The field, it seems to me, is one of the most alluring and promising which modern medicine and surgery present. It is certainly rich in promise for the many who, otherwise, would be doomed to more or less complete chronic invalidism.

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[17]See Fig. 9a.

DISCUSSION OF THE THREE PRECEDING PAPERS

Dr. L. A. Greensfelder (Chicago, by invitation): In sixteen cases where an abdominal operation was performed in the quiescent period, where we were able to carefully examine the ascending colon, in every instance a Jackson membrane was found which was not pathological, not causing any

symptoms of delayed mobility or obstruction. The membrane found in each instance had a definite upper and lower limit. I believe, from my own observations on these and other cases, that the reason why the membrane is not more often observed in children is, that it is a very fine, frail, web-like structure, which, unless one looks for it carefully, is very apt to be overlooked.

In adults I have not infrequently observed membranes of the Jackson type that have produced pathological symptoms,—constrictions, telescoping of the ascending colon,—where it has become pathological. Therefore, I believe that the Jackson membrane, normally occurs as a non-pathologic process, which may become pathological, the same as any other organ in the peritoneal cavity. I have never failed to find this membrane on the cecum of a child when it was carefully looked for. In children the ascending colon and the transverse colon are very mobile. Through a small McBurney incision you can, without any effort whatever, bring to the surface the ascending colon and a portion of the transverse colon, so that the observation can easily be made in children; and, likewise, I am quite confident that anybody who wishes to look for it carefully will find it in nearly every case.

Dr. F. Gregory Connell (Oshkosh, Wis.): As a contribution to this symposium, I shall give some unsatisfactory results following operations for these conditions. I have been able to secure recent reports upon 28 cases operated on during the years 1909, 1910, 1911. Of these 10 were unsatisfactory.

No. 1.—A satisfactory result for two years, then a miscarriage, which was followed by a return of her symptoms and the development of a pulmonary tuberculosis.

No. 2.—Previous appendectomy followed by no relief; secondary operation revealed typical pericolic membrane with double-barreled colon. Division of the membrane was followed by improvement for but a few months.

The above two failures were accompanied by five satisfactory cases occurring during the years 1909 and 1910.

The following 8 cases are accompanied by satisfactory reports from 13, all of which were observed in the year 1911, at which time I began a systematic search for these conditions in all operations in which it was advisable.

No. 3.—Usual symptoms with jaundice; gall-bladder, negative; division of pericolic membrane and ileopelvic band with

suspension of the cecum. Marked improvement, but during the present year the patient has had a gradual return of symptoms.

No. 4.—Interval operation for "chronic appendicitis," division of membrane; six weeks after leaving hospital an attack of pain in right iliac fossa, nausea, vomiting with an hysterical attack,—pseudo rabies.

No. 5.—Pozzi operation on cervix, removal of appendix, and division of membrane, with no relief of symptoms.

No. 6.—Appendix previously removed with no relief. At secondary operation pericolic membrane and ileopelvic band divided and cecum suspended, with no change in very marked symptoms.

No. 7.—Urinary-bladder symptoms, constipation, pain, and mass in right side. Tuberculosis, colon bacilli, and calculi excluded. At operation Jackson membrane, with marked atony and mobility of the cecum, division of membrane and cecopexy. Only temporary improvement.

No. 8.—During hysterectomy for large fibroid the appendix was removed; and the band causing marked Lane kink was divided. Symptom of pain in the right iliac fossa with constipation, worse after the operation.

No. 9.—Typical clinical history, membrane divided, and Lane kink corrected; followed by no improvement.

No. 10.—Clinical diagnosis of Lane kink, corroborated by *x*-ray, and demonstrated at operation; pericolic membrane likewise present; post-operative *x*-ray showed satisfactory result, but patient has the same attacks of severe colic, in all probability due to a Dietel's crisis.

During this time I find records of two cases in which a Jackson membrane was demonstrated at operation, but not interfered with.

Miss S. Removal of appendix, acute inflammation, with removal of gall-stones, followed by drainage of the gall-bladder; pericolic membrane distinct and definite, but not interfered with; no improvement.

Mrs. H. Clinical diagnosis of cholecystitis. At operation appendix normal, pericolic membrane present, no gall-stones; cholecystectomy, followed by no relief of symptoms.

Another case which may prove of interest, was not operated upon by myself.

Mr. C. First operation for appendicitis. After a year's interval was operated on for kidney stone, but there was no stone found. I first saw him after another year's interval,

when he was complaining of the same pain in the right side. I advised against operation; but he went elsewhere, and at an operation the colon was found fixed by "adhesions," which were divided, and the patient was well for just about a year, when once more he presented himself for the same pain. I again advised against operation, and instructed him to stay in bed for a week, during which time he was given petrolatum, and he has been entirely well for nearly a year.

Perisigmoid membranes were found in three cases, all of which made very satisfactory improvement after division and plastic closure of the peritoneal defect.

Dr. Leonard Freeman (Denver): Not long ago I made up my mind to observe, in such cases as it was possible to observe, the Jackson membrane. Since that time I have done 31 abdominal sections in which the membrane was studied. I found the membrane in 24 cases; hence, it has been extremely common in my experience. I have always looked for it; and I have been surprised when I did not find it, hence I am inclined very much to agree with Dr. Eisendrath that it is more or less of a normal structure, at least, in the particular cases in which I have had an opportunity to observe it. In these instances it was a thin, movable membrane filled, apparently, with blood-vessels and nerves, which looked as though it belonged there, looked as though it had always been there, but merged into Treves' membrane and into Jackson's membrane when these things occurred in cases. In but two instances was I convinced that it produced any pathologic effects. In one of these the obstruction was very marked, a case in which I helped another surgeon, Dr. Bagot, operate. The case had been operated on five years ago by Dr. Bagot for appendicitis, and the appendix removed; but the result was not what he hoped for, so we had to operate again, and this time he found a very thick membrane, thick almost as a piece of chamois skin, just below the hepatic flexure of the colon, which very evidently obstructed the bowel and which, when divided, liberated the bowel. The patient has been well ever since. Such a membrane, of course, is undoubtedly pathologic.

My assistant, Dr. C. W. Hall, made some 47 autopsies in the Denver County Hospital in which he looked for a Jackson's membrane. To my surprise, in spite of the number of cases in which I found the membrane in operating, he found it but 7 times in these 47 autopsies. I don't know what that means; whether it is a mere coincidence, or whether it means

that as people advance in age, Jackson's membrane has a tendency to disappear, because these 47 autopsies were all made in people past middle life; in fact most of them were quite old people. Is it not possible that many of these symptoms which we consider as obstructive symptoms are due to a drag upon the nerves in Jackson's membrane? If we accept this theory, is it not often sufficient to simply divide the membrane without having to strip it from the colon which leaves a very objectionable raw surface. I have done this in many cases where I thought possibly the symptoms were due to this drag. We might even in some instances fasten the stump of the membrane again to the abdominal wall in order to do away with the movable cecum.

In almost all of my cases my personal impression was that this was a form of congenital membrane, perhaps more developed than it was originally. When membrane is movable upon the bowel, it is not likely that it could have been due to inflammatory adhesions. There was no history of such inflammation in my cases. There was no thickening of the bowel remaining such as results from inflammation. We know also that when inflammatory adhesions appear, if the cause disappears, the adhesions have a tendency to disappear also. There was no inflammation apparent in the bowel; if it had ever been there, it had disappeared, so it is difficult to understand why this membrane had not disappeared also.

I feel that the whole subject is very far from settled, but at the same time I think that much light is being thrown upon it by what is being said here today.

Dr. Miles F. Porter (Fort Wayne): I think it should be clearly understood that the mere presence of what seems to be a rather unusual fold in the peritoneal cavity does not necessarily call for an operation of one sort or another upon that fold. Again, it does not seem to me that from a practical standpoint it is so important to determine whether these folds are of embryonic origin or of pathological origin as it is to determine whether or not they are doing any harm. It ofttimes happens, and has happened in my experience, too, that we find folds present that are doing a great deal of harm, but associated with other and more important pathological conditions. On one occasion, which comes to my mind very clearly now, after releasing these folds which were producing marked intestinal obstruction I very nearly closed the abdomen without any further search. Had I done so, I should have closed an abdomen containing a fibrosarcoma

of the transverse colon near the splenic flexure. So that when these folds are present and their ill results have been relieved by operation, we should bear in mind that they may not, after all, account for all the complaints of the patient. Of course, my experience has been extremely limited as compared with others; but I am not at all satisfied with the notion that these bands and folds are always either the result or the effect of a certain pathological process. That is, I do not think that it is proven on the one hand that Lane's kink is the result of stasis, nor do I think it is proven by any means that these kinks when they are present are the cause of all the symptoms that are present in these cases, and my basis for saying this is, broadly speaking, the same experience that Dr. Connell has had; that is to say, I have seen a number of these cases where the bands have been released and obstruction has been relieved, but the patients have been no better.

Dr. Willard D. Haines (Cincinnati): In the course of a year's work we have observed one case which we thought was producing symptoms. In that instance, it was as if you had tied a string around a coat-sleeve and produced an indentation in the colon, I should say an inch above the ileocolic junction, and then an inch above that. This patient was very markedly constipated, and we felt that probably by releasing these bands we might accomplish something in the relief of this condition. She made a very nice symptomatic recovery so far as the hospital was concerned; but, unfortunately, she was lost to view.

Dr. Van Buren Knott (Sioux City): I have made it a point whenever the abdomen was opened to ascertain if such a membrane was present. Dr. Eastman, at Cincinnati last year, gave us much information concerning this subject, and his demonstration had much to do in bringing me to the conclusion that this membrane was congenital in origin.

I simply wish to bring to your attention three cases which to my mind support this contention much better than any others I have seen, two mentioned by Dr. Eastman and one seen since. These were three cases of intussusception occurring in children, one at eight months, a second at eighteen months, and the third at four months, in which the colon beginning about the region of the ileocecal valve, to a point two or three inches beyond the hepatic flexure, was involved by the pericolic membrane. These children were referred

with no history of previous trouble, and the membrane could not be explained as having been the result of a pathological process.

I wish simply to emphasize one other point which has been brought out by Drs. Connell and Porter, namely, that it has been my experience that more harm than good, can result from injudicious attempts at handling of bands supposed to be responsible for symptoms.

I think the attention which has been called to this subject has created in the minds of many operators the impression that, not knowing exactly what is responsible for the patient's symptoms, if anything simulating one of these membranes is found it should be attacked, and is very frequently improperly handled. I have seen disastrous results from many such efforts. I think we should have greater familiarity with this subject before we know just in what cases we should attempt to liberate or remove the membrane.

Dr. Byron B. Davis (Omaha): Ever since Dr. Jackson read his first paper in Minneapolis, some five years ago, I have been looking for Jackson's membrane, and have found a good many of them. During the first two or three years after the report of his paper, I cut quite a good many of these Jackson membranes. During the last couple of years I have found that I have been dividing fewer of them than I did before. I have come to the conclusion that, although the Jackson membrane is a normal membrane and is present in a very large percentage of cases, in most cases, even if it is normally formed, it is really a conservative anatomical structure rather than a pathological one. I believe that in many instances it serves to fix the ascending colon and prevent a prolapse which would occur if this membrane were absent. I believe that when we get a pathological condition here it often does harm. The causes of any pathological condition in this location, I suppose, are very numerous. There may be chronic constipation, or an acute colitis, appendicitis with more or less involvement of the peritoneum; in fact, anything causing a peritoneal irritation or inflammation might cause enough increased vascularization and thickening of this membrane, so that later it would contract in such a manner as to bind down the colon, as I have found in a few cases.

During the last year I have been especially interested in these bands, because this committee had been appointed, and I wanted to make some observations. I have had just two cases in which I had a distinct ballooning of the cecum.

The latter was pinned down, being flattened by the membrane in such a manner as to cause a chronic obstruction to the fecal current, and in these cases I, of course, cut the band, and tried to relieve the condition. I am thoroughly of the opinion, however, that in ninety-eight cases, at least, out of a hundred, these membranes should be disregarded and not considered as pathological in any way. More than that: I am inclined to think that occasionally we have bands formed in this location that are absolutely and purely new formations.

I want to speak of one case which I had not long ago. This was a pus tube, acute inflammation of the right tube especially, and there had been a peritonitis, which seemed to be general. The physician in charge of the case told me that the woman had been sick four weeks. Finally, she was brought to me for operation, and I found a distinct band of adhesions running from the right side of the pelvis, running up, completely covering the intestine, almost up to the hepatic flexure, and it looked to be a new inflammatory exudation and exactly like the exudation we get between the coils of the small intestine following cases of peritonitis.

So I think the observation of the Jackson veil should be continued, but that we should be very sparing of the knife or scissors in cutting this membrane unless it is absolutely producing intestinal stasis; and I believe it is only in a very small percentage of cases that it produces intestinal stasis.

Dr. Arthur A. Law (Minneapolis): This discussion is a wonderful tribute to our president, Dr. Jackson, and is a valuable contribution to the subject. We have operated upon 50 cases in which the membrane was found. They were all studied with the fluoroscope after giving a bismuth enema; and all had stereoscopic plates made. We are of the opinion that the *x*-ray findings are of no practical value, except when they show evidences of obstruction in the large bowel. Only if ptosis is present is the *x*-ray of value in determining the position of the bowel. Every one of our cases had a ptosis of the cecum. The latter was either in the pelvis or descended there when the membrane was released. Cecalatony was also present, and was so pronounced sometimes that the bismuth meal did not get into the transverse colon until forty-eight hours after ingestion.

We have not found the pericolic membrane to occur commonly in our clinic, and inquiries directed to the chiefs of the twenty-five largest clinics of this country revealed the

fact that only two (Dr. Cheever of Boston and Dr. Summers of Omaha) had seen this membrane in children. Among our 50 cases we had 8 who were twenty or under, 1 of fifteen, 3 of sixteen years of age; 38 out of the 50 were women, and 12 men.

Dr. Eastman's experiments on rabbits are illuminating. Our personal conviction has been crystallized into the idea that this condition is secondary, practically in almost all our cases, in any event, to fecal stasis. Dr. Eastman has produced fecal stasis in animals by bringing about practically the same condition, and membranes about the colon resulted.

In conclusion, I would like to ask, Has any one seen records of recurrence after removal of this membrane? Geister claims it recurs, but does not quote any statistics to confirm his statement.

Dr. William T. Reynolds (Kansas City): Dr. Eastman's paper was read last December at Cincinnati. I made up my mind to follow up some of his observations upon investigation of the inside of the belly of the fetus. I have had eleven of these, the youngest of which was six months and the others up to term. In no instance was there an absence of this membrane on the right side of the colon high up, and in no instance was it absent on the terminal ileum: In 3 cases it was present on the opposite side, as observed by Dr. Eisendrath. Observation during a considerable number of laparotomies in the past year has been made by me with special efforts to look for this membrane, and I tried to analyze the situation as to whether I should disturb it or not. I have arrived at the conclusion that, unless the membrane or the constricting bands produce a definite kink in the intestine at some place, I do not disturb it. If it is a smooth, glistening membrane involving the intestine, but not producing a dilatation of the cecum, I do not divide it. If it is constricting the ascending colon, I simply divide it and free the colon. We do not know what the end-results are.

Dr. Archibald A. Kerr (Salt Lake City): I have been observing this condition for several years and have seen it on numerous occasions. The papers which were presented at this Association last year and the paper of Dr. Cheever at the A. M. A. meeting, have thrown much light on the subject. The cases that I have personally seen have been associated with intestinal stasis or chronic appendicitis.

I might relate the history of one or two cases, briefly, just

to illustrate what I have said. One was a young woman recently married, twenty-six years of age, who gave a history of chronic constipation. She was operated on about six and a half years ago by the late Dr. H. D. Niles. The appendix was removed, but two and a half months later her symptoms recurred. She had pain in the right side, referred to the back. She had very obstinate constipation. I re-operated on her and found a typical Jackson's membrane extending from the cecum up to the hepatic flexure of the colon. I removed some of the bands, which, in my own judgment, were producing obstruction, but did not disturb the most of the membrane, a policy which I followed for several years. She had considerable, but not complete, relief from the operation. I had a supporting bandage made, and gave her liquid petroleum, as suggested by Dr. Connell, and that seemed to relieve her intestinal stasis better than anything that I have done, and she is at the present time very comfortable.

Another case which I might briefly relate was a man forty-eight years of age. He had symptoms of chronic appendicitis, very little fever, a great deal of pain over McBurney's point and chronic constipation. I operated on him. I also found a typical membrane extending from the base of the cecum up to the hepatic flexure. It is too early to say what the ultimate results will be in that case, but up to the present time he has been greatly relieved.

Dr. Clifford U. Collins (Peoria): It has been interesting to me to see the unanimity with which the reports have agreed on certain phases of this question. They could not have known what each would say, and yet they have all agreed. I have formed some definite conclusions regarding the pericolic membrane.

For a short time I removed every Jackson veil I found, whether the patient had complained of symptoms referable to the membrane or not. It was found in 55 patients. Sterile vaseline was applied to the raw surface left by the separation of the membrane.

The experience gained in these cases has led to the following conclusions:

1. The symptoms caused by the pericolic membrane, such as pain and soreness, are not relieved by the simple removal of the appendix. In 6 of the patients the appendix had been removed before, without relief of the symptoms; but the symptoms were relieved after the membrane was separated.

2. If the membrane is causing symptoms its separation or removal will usually relieve the symptoms. In 25 patients who complained of symptoms referable to the membrane, the symptoms were relieved, and the patient benefited in all except 2.

3. If the membrane is not causing symptoms, it should not be disturbed. Some patients have complained of pain and soreness in the region of the ascending colon and cecum after the veil had been separated, who did not complain of these things before the operation.

4. A dilated, movable cecum is usually associated with a constricting veil of the ascending colon, and should be plicated at the time the veil is separated. Six of my patients had a cecum mobile. All complained of pain and soreness in the region of the cecum before operation, all had a constricting membrane of the ascending colon, and all were greatly benefited by the plication of the cecum and the separation of the pericolonic membrane.

5. The application of sterile vaseline to the raw surface after separation of the membrane does not always prevent adhesions from forming. Two of my patients were operated on again some months after the membrane had been separated, and dense adhesions of the ascending colon were found. The original symptoms, however, were relieved.

6. Constriction of the ascending colon by a Jackson's veil causes symptoms, sometimes, which very much resemble those caused by cholecystitis. If a diagnosis of cholecystitis has been made and the gall-bladder is found to be normal the ascending colon should be examined to see if it is constricted by a membrane.

Dr. Arthur N. Collins (Duluth): It is not an easy point of art to disentangle ourselves from this riddle. In opening the abdomen I have found a vascular film or veil of greater or less extent in at least 75 or 80 per cent of cases. How many of these are pathological in the sense that they are giving, or will in the future give, signs or symptoms? This is a difficult part of the riddle.

There are two features upon which the clinical factors of constipation and attendant systemic and local manifestations depend. These are (1) the extent and (2) the thickness of the membranous veil.

In my experience, covering some 25 or 30 cases, in which the membrane was dealt with, the most frequent location of

the veil was upon the ascending colon, next on the cecum, and the hepatic flexure alone coming third.

I am not prepared to answer the question as to whether the fold of Jonnesco persists throughout life, nor am I concerned with the uncertainty of opinion as to whether Jonnesco's fold is one and the same with Jackson's membrane. If an enveloping membrane exists in infancy—and Eastman's studies show that it does in a certain proportion—it probably persists into adult life.

It is my belief that the membranous veil is intimately concerned in cases of acute or chronic constipation or stasis in the large bowel. By treating the membrane surgically, I have relieved the constipation more often than the pain and tenderness.

There is still an important question which we have to answer: "Is the veil the cause of the constipation, or is the constipation a factor aggravating an already existing veil; or have we to deal with a peculiar type of individual in whom this membranous veil is more prone to occur and become pathologic?

Wilms' experience with cecopexy, by which he relieved the pain in all and the constipation in 75 per cent of his cases, led me to use the method I have diagramed on the charts. The membrane is tied fast in several places, making stumps and leaving the membranous part of the stump still attached fan-like to the colon. Before closing the wound, one or more stumps are anchored to the anterior abdominal wall at points corresponding to the situation of the stump upon the gut.

Dr. Malcolm L. Harris (Chicago): A little over a year ago, at the Atlantic City meeting of the A. M. A., I presented a paper in which I tried to call attention to the various forms of adhesions and membranes which might be found in the right side of the abdomen. To be brief, I mentioned what could be classed as pure inflammatory adhesions, such as we see in any part of the peritoneum as the result of infection, such as occur so frequently about an inflamed appendix, about inflamed pus tubes, etc. Then we find what I prefer to call agglutinations of the peritoneal layers, which I believe are due to a failure to perfect the agglutination and disappearance of the two layers of peritoneum, such as we know occurs normally in this location; and this failure to go on and perfect this process may simulate peritoneal adhesions; the nature of which, I think, can be very easily recognized. Then we have membranes which are undoubtedly of congenital

origin, such as have been so well described by Dr. Eastman; and membranes which, I think, are undoubtedly pathological in origin, and due to the transmission of low grades of infection through the bowel wall, such as have been produced experimentally by Heide; hence no one explanation can account for all of these membranes and adhesions. Many of the adhesions are agglutinations, which give rise to no symptoms at all, and, I think, many of the adhesions or membranes which are undoubtedly of congenital origin give rise to no symptoms for many years, and then eventually become pathologic in the sense of causing symptoms; and they cause symptoms, I believe, because of changes which have taken place in them as a result of the absorption of toxic products from the bowel, changes which have taken place in the location of the intestine, such as a sagging of the bowel from chronic constipation, etc.

Of all the explanations of the cause of these membranes with which I am familiar, that of Mr. Lane is, I think, the least probable. His "lines of crystallization of force," as Dr. Eastman has said, to me are absolutely meaningless. To think of lines of force giving rise to membranes in this way is something that I have no knowledge of in anatomy and pathology. That these membranes existing as congenital conditions, in the presence of visceral ptosis or in the presence of intestinal stasis producing a low infection, causing atony of the intestinal wall with sagging, may become the secondary cause of pathological symptoms, is to my mind reasonable; but that a visceroptosis should be the cause of the development of these membranes to hold the bowel up is to me the least satisfactory explanation of any that have been spoken of in reference to these membranes.

I would like to call attention to the fact that after these membranes are dealt with the patient becomes a medical case; and unless properly treated these membranes may recur, due to pathological states or low forms of infection in the intestinal canal. I have never been able to get away from the most convincing experiments of Heide in the production of these membranes by a low form of infection from the intestinal canal by anaërobic organisms. We can produce experimentally these conditions in animals; and when we find these same conditions existing in the individual, and when we know the great frequency with which intestinal stasis takes place, in my opinion, this low infection from intestinal stasis is the underlying factor in this condition.

Dr. Thomas C. Witherspoon (Butte): This subject is certainly full of interest, but not knowing I was to be called upon for a definite expression of opinion, I have prepared no paper on the subject.

In this discussion an effort is apparently made to fix upon some definite cause which will universally account for the presence of a Jackson veil, a Jonnesco, Treves, or Reid band, a Lane kink, or mesenteric thickening, with shortening, and possibly peritoneal adhesions, wherever they occur. To me, however, it seems that in the matter of pathological adhesions, Jackson's veil, or toxic thickenings in band or mesentery, we are dealing with a process that has to do with absorption of some chemic body out of the intestinal tract; whereas a Jonnesco, Treves, or Reid band, and the union of the colon with the posterolateral abdominal wall, are merely physiological processes which cannot be considered in the same category. That the colon should fuse with the abdominal wall, upon which it rests posterolaterally, and not with the bowels, for they also come in contact, is a condition difficult to understand, but certainly physiological. Where, however, a Jackson veil is discovered, or a marked thickening in one of the bands of the mesentery, there is always evidence of toxic absorption, congestion, and interference with the normal mechanism of the bowel. That the chemic agent which produces this pathological irritation is always the same, or that there may be a number of agents, has not been definitely proven. It is strange that some cases of pendulous, dilated cecums are thoroughly inveiled by a Jackson membrane, and others, quite as distended and pendulous, are not. This would seemingly point to some specific chemic agent, plus the mechanical.

That certain portions of bowel only are capable of fusing with the wall, physiologically, is not wholly true, as illustrated by an interesting case that came under my observation four months ago, in a young woman, whose cecum I found to the left of the median line, attached to the posterior wall by a long mesocolon. In this patient the stomach was much too far to the right. The duodenum passed directly down from its distal end, in about the normal location of the ascending colon. The duodenum was fused with the wall at this point, just about as is the colon normally. From the pelvic brim the duodenum passed onward, attached by its usual mesentery, and occupied the lower right quadrant of the abdomen. The ileum was located entirely upon the left side of the

abdomen, and ended in the cecum, which swung freely and hung low upon the left side. From the cecum the colon passed upward to the under border of the stomach, then to the right along the greater curvature. Under the pyloric end of the stomach it turned abruptly backward; then again to the left as far as the normal position of the splenic flexure. From there the descending colon, sigmoid, and rectum seemed normal in position and appearance. There was considerable narrowing of the colon under the pyloric end of the stomach, where it turned backwards, and the cecum manifested this obstruction by dilation and a pendulous position. There was, however, no sign of a Jackson veil to be found.

This case presented two conditions, which may help to throw some light later upon the subject with which we are dealing: in the first place, the union of the duodenum with the wall in the usual position of the ascending colon; and, in the second place, a dilated stagnant cecum in which no Jackson membrane occurred. The latter can be accounted for only by the fact that the freely moving bowel does not so readily allow of this veil development, or that the specific toxic element which produces a Jackson veil was absent from this patient's bowel.

That freedom from active motion seems to be necessary in the formation of these veils would seem apparent from the fact that the small bowel rarely is covered by it. In chronic pelvic inflammations, after the bowels and uterus have become adherent through the inflammatory process, these may be stretched by the effort of the bowel to move, or through the accumulation of inflammatory serum, and then we have veil-like formations. These are, however, not vascularized, and do not present the appearance of a Jackson veil as found about the colon.

It is possible, also, that the small bowel manifests its freedom from this Jackson veil because of the fact that its contents are so fluid that it usually passes entirely through it in six hours. In the big bowel, however, it may remain for the next eighteen to seventy hours, and cause considerable more irritation.

In children this veil-formation is not so common, for the reason that they are more active, and stasis of the bowel is not met with so often, and, in fact, if existent, has not been so as long as in the adult. When, however, they give up their play and exercise, and the alimentary contents become more

stagnant, the toxic absorption may manifest itself by this veil-formation.

I have not had the opportunity of seeing, by secondary operation, a cecum and ascending colon, which, pendulous and covered with a Jackson membrane, had been operated upon formally after the method of Wilms. It would be interesting to know whether, after anchoring the big bowel and giving it better support, with less stagnation, this membrane disappears or still remains.

This is a subject that at best seems unsettled at present; and I for one feel that I have much to learn about it, and know so very little about it at present that I speak only because I am called upon to speak, and not because I have something worth while to add to our knowledge of it.

Dr. Jabez N. Jackson (Kansas City): I must express my appreciation of this very extended discussion this afternoon. It is a matter of some surprise. I am convinced that there are many cases reported of pericolic membrane which do not deserve that name at all. Some have gone so far this afternoon as to say this membrane was normal, that it existed in everybody, which is a statement in itself, I think, absurd. At the time I presented this paper, a great many men said they did not think such a thing existed at all, and now we find them saying it is found in everyone.

In fact, I think we do find that there is such a thing as this pericolic membrane, that it does exist. At the same time something resembling it, the analogue of the pericolic membrane as I described it, in a non-inflammatory stage, exists frequently. In the paper which I presented at that time my remarks were based entirely upon clinical observations of patients operated on with specific clinical symptoms. It is quite a different thing from many observations which, unfortunately, are obscuring the real condition, observations made on every patient operated on, regardless of symptoms. I am quite frank to say that in the routine method of examining the colon that has come into vogue with myself and others the last few years, I have often demonstrated what is called the Jackson membrane in a great many patients who have no symptoms on the right side; and it is not pericolic membrane at all, and is absolutely entitled to be passed by without any consideration at all. If a man simply demonstrates a few longitudinal bands on the right side of the colon, calls that a Jackson's membrane, and proceeds to strip it off, he is

certainly doing vastly more than I, for one, had any intention of advising when I presented the subject.

I am not satisfied as to what is the origin of the pericolic membrane, but I am satisfied as to the symptoms which it does produce. I will put it this way: I have seen a vascular membrane which might be called a pericolic membrane in a great many patients who had no symptoms; on the other hand, I have never operated on a patient who had these classical symptoms that I did not find it. In other words, the relationship of these symptoms is definite; you will find this doing what the symptoms indicate. When this exists as a clinical entity, it will always be found interfering with the mobility of the gut, and will absolutely explain the symptoms; and if it does not it should be absolutely ignored. If a person simply finds a loose, vascularized veil floating over the colon, with no bands of fibrous tissue in it, and not definitely interfering with the capacity of that gut somewhere, he should just dismiss it from his mind.

Now, we believe today that the observation of practically all cases shows that this membrane exists. In certain cases, whether it be due to the congenital origin of these membranes, of which Dr. Eastman speaks, or whether, as Lane says, "crystallization of lines of force," has taken place, or as Witherspoon interestingly says "the mark has been overshot," we believe that if Nature has endeavored to hold up the colon, as presumed by Mr. Lane, that Nature has overdone the job and produces, thereby, a pathological condition, or, as we say, "it is overshot." Everybody has a tendo Achillis, but that does not signify that everybody has got a club-foot. Only when that tendo Achillis has contracted, do we need to operate; and only when this membrane interferes with the mobility of the gut do we need to make it a consideration. Whatever be the origin of that vascular veil, it is not entitled to clinical significance until we can demonstrate that in the individual case it is something that will explain the symptoms.

It is interesting, also, to note that a good many men have reported cases here that have been operated on with marked relief; and then in the course of possibly a year or two, more or less, there have been recurrences. Now, the question comes up, why? The question again arises as to whether we have as yet discovered the true, logical method of treatment of these cases. I am very glad of one thing; and that is the way I presented this subject originally. I have nothing to apologize for to anyone, because I did not make any argument: I simply

stated my observations, and offered as a suggestion the idea that this membrane was interfering mechanically with the capacity of the gut, and should be divided or stripped off; and this I would say depends upon the individual case. In some instances a mere slit of an incision is all that is necessary. Sometimes the attachments are so multiple that no simple line of incision will do it, and we have practically to strip the restricting membrane off.

Therein is where the most interesting thing comes in, in my judgment, in reference to the really correct treatment of this condition, and that is that the etiological factor is of importance. Thus, if we are dealing with simply a congenital defect, where Nature has overshot itself, and we simply have mechanical problems to correct the over-action of Nature, if it is simply in this membrane outside of the gut and nothing more, why, then, we have nothing more to do than to deal with the mechanical consideration. If, on the other hand, this condition is an external reaction from an internal condition, whether it is a colitis, whether it is, as Dr. Haines said, a passing through the gut-wall of certain anaërobic bacteria with reaction through starting up a chronic inflammatory condition and formation of fibrous connective tissue, it is different. You can readily understand that, if that is the true etiologic factor, anything we do on the outside is going to be probably wasted effort. If the cause still exists on the inside of the gut we can expect a reproduction of the symptoms.

I am frank to say that up to the present time in working out the problem logically for my own satisfaction, I have chosen the congenital or mechanical viewpoint. I first simply divided these constricting bands or stripped them off. I found certain cases relieved entirely, and some where there was no relief. Then I began to recognize the existence of another almost constant factor. Here you have a pericolic membrane, so-called cecum mobile of Wilms. I realized that one must look upon the cecum as a propelling bulb, which starts the circuit off. I have seen constriction in the colon until the cecum became so mechanically distended that it could not contract. In other words, if I put your arm in a plaster cast, and keep it there so that the muscles have undergone atrophy, I could take off the cast which was the cause, and yet you would have no use of the arm. I have begun to believe that in some of my cases failure was due to the development of atony of the gut, and hence the trouble.

I took up the next step. It did not appeal to me to do the fixation as Wilms does it, i. e., to put it in a pocket of the pelvis where it will interfere with motility and possibly pregnancy in women. The other idea did appeal to me, and for a year I have put in a transverse plication, turning in about one-fourth inch depth on each side of the anterior longitudinal band, and reducing the size of the cecum in that way. I must say that my results have been vastly better than before, so much so that in none of these cases have I had an entirely unsatisfactory result. But to be perfectly frank, it is curious how things run in bunches: in the last six months I have not seen a single case of membrane or of pericolitis clinically.

I believe, therefore, as a summary or conclusion, that we have a condition which exists, and in certain instances is associated with certain symptoms. If all this discussion did not teach us anything more than the fact that we have been doing a whole lot of operations for appendicitis that were useless, we shall, at least, have got that much further along the line of positive knowledge, and know what to keep away from in certain individuals. I believe, on the other hand, this condition has a positive relationship to symptoms, that it is a positive condition with positive symptoms, and that we shall finally solve the question accurately with vast benefit to our patients. But we must not interfere in the cases that have no symptoms associated with them.

THE BEST METHOD OF SUSPENDING THE RIGHT COLON

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The best method of suspending the colon is the method that best overcomes the bad effects of a displaced colon. These bad effects are—

First. The right colon when displaced downward interferes with the lumen of the duodenum where it passes under the colon just below the normal hepatic flexure. Dilation of the first and second part of the duodenum follows, with retention of bile, regurgitation, and bilious vomiting.

Second. The ileum, a few inches above the ileocecal valve, is quite securely fixed by the junction of the mesentery of the ileum and the meso-appendix. When the right colon descends two things follow: a Lane kink forms by the sharp bending of small intestine at this fixed point, and the appendix is inverted, becoming a suspensory ligament to the cecum. Acute or chronic appendicitis follows; and the operation of appendectomy, always in my service, accompanies the operation of cecopexy.

Third. When the colon is loosened it easily swings towards the center of the body owing to the slope of the pelvis. The mesentery of the small intestine passes obliquely from right to left, beginning at the ileocecal junction, and passing upward toward the splenic flexure. When the cecum swings downward

and inward, as it does in these cases, the twenty-odd feet of small intestines, which hang from this one foot of mesentery, are crowded upon themselves in close folds. When the head of the colon is swung upward and outward to the right side, this bunch of small intestines is opened by the movement of the cecum outward and upward. It is as though one held a fan partly opened and attached a tube to the end of each spoke in the fan. When the fan is closed this is kinked and cramped upon itself; when the fan is opened wide, the tube becomes straightened. By attaching the cecum rather high on the right side, we "open the fan," so to speak—we restore the mass of small intestines to normal position. In this position it can functionate properly, which it cannot and does not do in the crowded or massed position that accompanies displacement of the colon downward and inward.

Fourth. When the head of the colon becomes loosened and descends and passes inward, causing the kinking and the massing above described, the patient becomes liable to one of the most dangerous of abdominal conditions,—the wrapping of the intestine upon itself from right to left, i. e., volvulus.

The right side of the ascending colon is the only portion worth suspending. The transverse colon attends to itself if the hepatic flexure is made right. The splenic flexure seldom descends. The sigmoid is not included in this paper.

We understand, then, that we suspend the right colon—

First, to cure obstruction and dilation of the duodenum, with retention of bile, and pancreatic juice, regurgitation, and bilious vomiting.

Second, to forestall appendicitis.

Third, to restore the lumen of the ileum shut off by Lane's kink, and to restore its functional activity.

Fourth, to prevent volvulus of the small intestine.

Why we suspend it being considered, the next question is *where.*

The large intestine does not pass vertically upward, but upward and forward. It is essential that the head of the colon be fixed, and that more and more motility be given up to the splenic flexure. The colon falls forward by its length of attachment, and the hepatic flexure is well forward in the abdomen. We respect the anatomical fact in putting in our suture line, setting those sutures which fix the head of the colon deep at the back of the fossa where the caput coli belongs, and each successive one a little higher and further forward toward the front of the body. The "why" and the "where" being agreed upon, the "how" becomes a matter of taste and technic. Whatever procedure is used inside the abdomen, it is essential that the normal pressure of the anterior abdominal wall be secured.

To place the sutures I use a full-curved needle, small in diameter, long and sharp-pointed, without a cutting edge, for the first suture deep in the abdomen just above the iliac crest. One of lesser curve, but otherwise a similar needle, implants the next one or two; and a long, straight, intestinal needle implants the sutures near the abdominal incision, and is easy of manipulation.

The suture material has been Pagenstecker thread, which we have lately abandoned for No. one, chromicized, twenty-day catgut. The suture is placed well back in the abdomen just above the iliac crest. For the first suture the needle passes deeply into the tissues and out again one-half to one inch toward the spine, then the needle is passed in and out of the bowel-wall close to the stump of the appendix, catch-

ing up the peritoneum and muscle of the bowel-wall for at least an inch, and at some point of the inch securing a firm grip upon the lateral linea muscularis. Then the operator hands the needle to the assistant upon the left side of the table. While the operator retracts the opening of the abdomen he pushes inward the wall where the suture is to be placed. The assistant pushes the needle deeply into the side, from one to one and a half inches above and in front of the first hold of the suture, and comes out near the loose end of the thread. The ends are held in forceps. While the operator on the right holds up the abdominal wall the assistant, under direction of the operator, draws several scarifying lines along the side from the edge at the upper end of the abdominal incision in the right linea semilunaris downward and backward to the first suture. This marks the site of attachment of the colon; and four to seven sutures are used at intervals from one to one and a half inches to secure it. Adhesions that close or seriously lessen the lumen of the colon are cut away. The masses or bands of red, veil-like tissues variously named are, if present, let alone and disregarded. When all sutures are placed they are tied.

A sharp reaction comprising pain, gas-distension, soreness in the right side, and temperature usually appears the second or third day. We attribute this to an adhesive, post-operative, conservative pericolitis.

The patients are given a colonic flushing the fourth day, and a carthartic after the first week. When dismissed they are given Russian paraffine oil for an indefinite period to lubricate the bowel. The diet is about 1,800 calories of food, carbohydrates predominating and proteids sharply restricted.

Dr. Donald Macrae (Council Bluffs, Iowa): If I had the gift of oratory that many of these gentlemen possess, I would like to say something strong, but I find all that I can do is to say that I am absolutely opposed to the methods that Dr. Pearse has advocated. I think it all belongs to the same territory to which the discussion on pericolonic membrane belongs, to the class of cases which have to do with what I would term fecal retention. We have the transverse colon demonstrated very nicely by *x*-ray after the bismuth injection, and find it does not completely empty itself sometimes for days, and yet the patient's bowels will move daily, and this will be visible in the transverse colon.

Prior to a couple of years ago, it was my privilege to attend a great many of these operations, especially after our illustrious President's thesis before the Association on pericolonic membrane. It was my privilege to operate on a number of these cases. Some of them did very nicely, but I thought that some of them later had symptoms similar to Dr. Connell's. For the past two years I have had associated with me an interne who has used an enormous amount of paraffin oil, followed with laxatives, with attention to diet; and I find that a great many of my cases I formerly operated on are getting well without an operation, and I cannot help thinking that many of these cases are primarily medical, and not surgical. It seems that it is a question of stasis. I believe that this last paper illustrates, the same as the previous papers, the possibility of the fact of a previous condition existing, possibly a fetal condition (Dr. Eastman has very clearly and positively shown that it is found in the fetus), which by reason of fecal stasis later becomes a pathologic entity. Now, nearly all of these cases are found in women, I think Dr. Law reported probably about 90 per cent of the cases occurring in women, and I think it is true with you all. Women, as you know, lead sedentary lives; they do not care for their bowels. But it seems to me that by using paraffin oil, appropriate laxatives, and proper dietary regime, we would not have so many operations for diseases, pericolonic membranes, etc. I am convinced of that because it has been followed out absolutely in our practice. I do not mean to detract from anything that has been said. I think it has been a magnificent discussion. I thoroughly believe in what has been said, but I think it is going too far. I certainly do not believe in tying up the cecum. I believe the cecum is dilated because of an obstruction, or what-

ever it may be. Remove that pericolonic membrane, if other measures fail, and see that the fecal current is kept up. I believe it would be a great deal better to get it going before the operation, but, if necessary, do the surgery and then continue your medical treatment. I am convinced of that. I don't believe in tying up the colon, I don't believe in taking the colon out, and I don't want to go on record in this Association as advocating these procedures in the vast majority of cases.

Dr. Pearse (closing): I said we had been so drenched with pathology that I thought about four minutes of technique would be enough for this paper. Now, I have to go back into pathology in order to get into my friend's head that the fixation of a colon, which has reached the pubic bone, is necessary. It is terminal pathology. It has nothing to do with cascara and paraffin oil, and must be dealt with as a case of terminal pathology. It occurs after the family doctor has failed to give the patient the treatment he should have had long ago.

Before I got through I said that when you fix the head of the colon, it is necessary then to restore the support of the anterior abdominal wall. To be patient, to be brief, I would say that in the treatment of constipation there are several steps: First, the family practitioner, who should see to it that the constipation is cured by means of cathartics, etc. This is one step. The next step belongs to the general practitioner or to the surgeon or the gynecologist or whoever is so fortunate as to get the individual in his hands when he finds it necessary for the patient to constantly take a cathartic. Begin by external support of the abdomen, by giving proper oils and muscle stimulating cathartics and by proper diet, and endeavor for six, eight, or ten months to obivate a terminal pathologic condition. Here is where my paper begins, with a condition of dilated right colon that is dilated until the woman is lying in bed with an immensely enlarged colon that has dragged on the appendix, using it as a suspensory ligament. Then you open the abdomen, and find a movable colon. Then you have reached the surgical, or last, stage of constipation, of intestinal stasis, of repairing the terminal pathologic condition that is permanent. The doctor's discussion was on the first two stages. My paper deals with the third stage. You can do one of three things; you can short-circuit the ileum to the sigmoid, or you can remove the colon entire. Both of these I have done many times before I abandoned them, but I found since doing the simple procedure, not in every case

of constipation, not in cases needing diet and cathartics, but in cases of intestinal paresis with a widely dilated colon and damaged anatomy only on the right side, that if you lift back the colon, fasten it securely there, and for a few months give the patient the really therapeutic treatment, he ought to have had years before, you can effect a cure without changing the current and without the great amount of shock and danger involved in the entire excision of the colon, by the simple procedure of carefully and scientifically and accurately fixing the right colon on the right side, rather high up and well forward and supporting the external abdomen afterward. Did I make myself clear?

Dr. Macrae: I don't understand it at all.

CHOLECYSTITIS AND THE FACTORS THAT CONTROL RESULTS OF OPERATION

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ROCHESTER, MINNESOTA

The surgical treatment of diseases of the digestive system is now a common procedure. The symptoms which are directly attributable to the stomach and those of a reflex character from other causes so commonly referred to this organ, assume great importance in diagnosing diseases of the abdominal organs. This is especially true of those diseases which are concerned in the digestion and preparation of food for absorption. However, it is often difficult to distinguish true gastric symptoms from those that may be caused by remote conditions in the abdomen, as well as those influenced by the general health and the mental status of the patient. Direct observation of the transitional changes caused by disease of the organs concerned in digestion, together with a correlation of facts obtained at autopsy, has added greatly to our knowledge regarding changes in secretion, the erosions and ulcers, as well as the benign and malignant tumors of the stomach. Undoubtedly, within the next few years accurate knowledge will be obtained of the disturbances which lead to changes in the acidity and the conditions which lead to the ulceration of the stomach, as well as of the duodenum. Even now, in the consideration of digestive disorders, many look upon gall-stones as the source of serious trouble only

when colic occurs. Our knowledge of the development of these diseases shows that not only the local infection may cause symptoms, but also that they may be due to secondary obstruction or derangement of the pancreatic secretion, with consequent change in the digestion and delivery of the chyme from the stomach itself.

Some of the important factors in the production of disease are those infections which derange the alkalinity of the duodenal and pancreatic secretions, especially by infected bile. The acid chyme from the stomach is intermittently emptied into the bowel, a process which Pawlow has shown is accomplished and repeated as neutralization of the acids is completed. Thus conditions of temporary tension in the stomach through so-called pyloric spasm, may be produced by derangement of the balance of the alkalinity of the neutralizing secretions. It is well known that pyloric spasm, and even regurgitation into the stomach from the upper intestine, may occur from interference with peristalsis of the small bowel, as well as from irritation of the appendix.

The liver and its biliary secretion are influenced by the invasion of bacteria into its ducts and into the gall-bladder, and also by obstruction due, not only to its own diseases, but to the secondary changes of the pancreas and duodenum, and to adhesions from local infections outside its own tract. Our knowledge of the function of the pancreas, as well as of its diseases and influences on digestion when deranged, is slowly increasing. At present we are sure only of the gross changes in these organs. It is still difficult to diagnose clinically, with the abdomen open, between some cases of cancer of the pancreas and a lymph-edema from infection and obstruction. Changes in the liver must be gross to be distinguishable. This is but reasonable, since nature has provided such large

Fig. 1. Fat protection of area of gall-gladder.

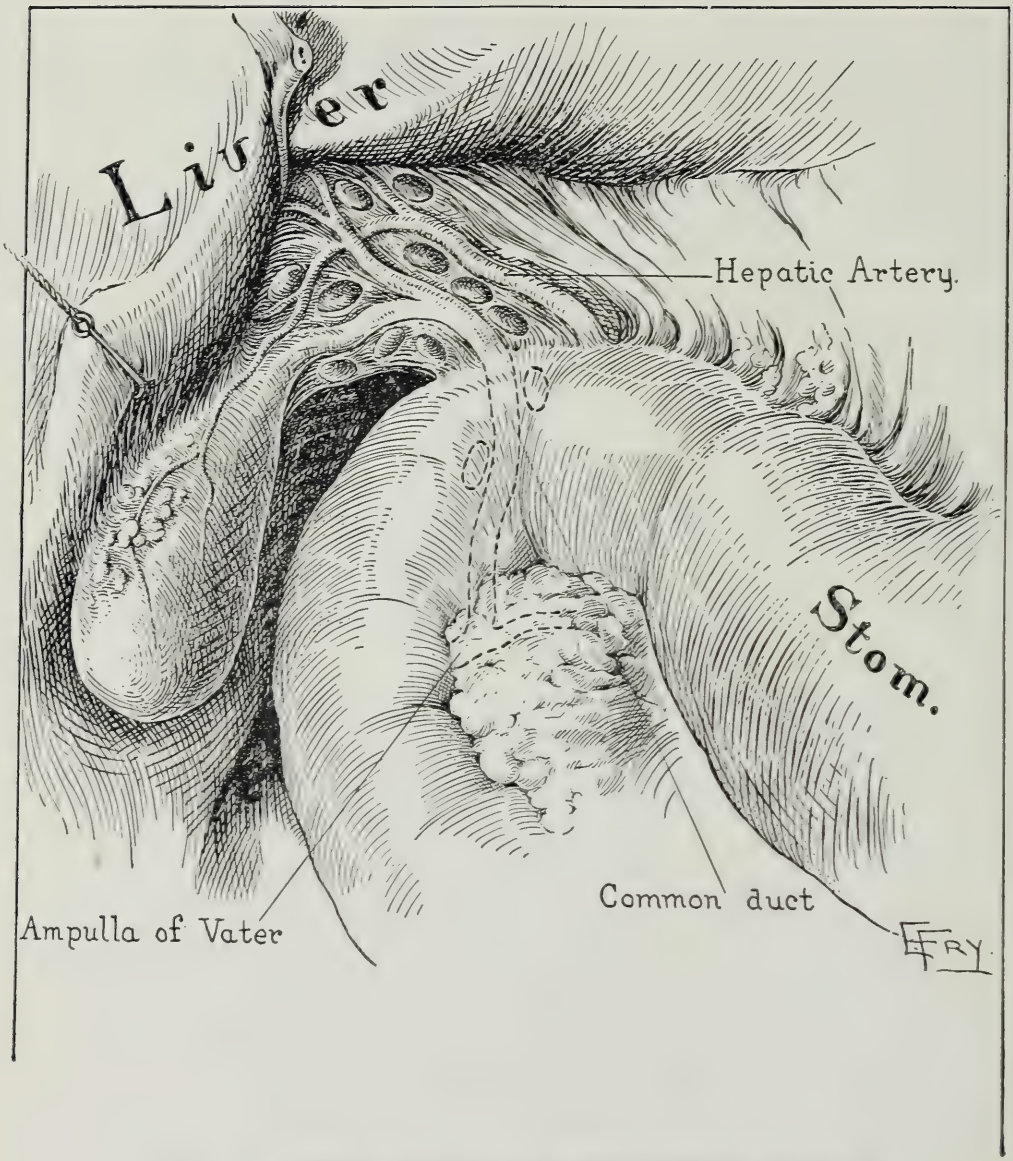

Fig. 2. Glands draining the gall-bladder, head of the pancreas, and duodenum.

factors of safety in these great digestive glands that, if but a portion be functionating or if the function of the organ is not too greatly changed, there may be but few recognizable symptoms. Thick mucus in the gall-bladder may occasion considerable colic, while the infection incident to cholecystitis and gall-stones may result temporarily in a greatly reduced acidity of the gastric fluids. In most gall-bladder cases much gas is noted early following the taking of food. This condition is worse in periods, but seldom entirely absent. Moreover, reflex gastric symptoms are nearly constant but of varying severity, and do not occur in marked attacks or spells, as noted in true ulcerous conditions, although both symptoms may occasionally be found existing in the same individual.

Another important diagnostic point is qualitative food dyspepsia, in which certain kinds of food, especially apples, cabbage, greases, etc., shortly after ingestion, occasion distress, colic, and gas and eructations. This is in contradistinction to quantitative food distress, averaging two hours after eating, which occurs in cases of intestinal kink and chronic appendicitis with concretions, and which is caused by interference with peristalsis.

It is necessary before an operation is undertaken carefully to weigh the evidence of the various symptoms which are believed to be caused from the diseases involving biliary excretion. In many cases it is impossible to differentiate between gall-bladder disease, duodenal ulcer, and appendicitis. Often also more than one disease is present. However, all of these condition are surgical; and exploration is indicated in the majority of cases. Yet, the more the symptoms of these serious conditions overlap, the more probable is the foundation purely nervous, and the less probable that operation will benefit the patients.

The importance, then, of being able to diagnose cholecystitis when the abdomen is open, is of great moment. It is usually easy enough to identify stones and infections of the gall-bladder secondary to obstruction. In cholecystitis, however, serious infection, with foul-smelling bile and "straw-berry" mucosa, may not change the normal blue appearance of a thin-walled gall-bladder, and may not cause adhesions about it.

In many such a case the abdomen has been closed without the true condition having been recognized. The main object of this paper is to call attention to the means at hand with the abdomen open, which should lead the operator to a correct diagnosis of cholecystitis and prove the presence of a diseased gall-bladder, or force him to search for disease of some other organ.

I wish to call attention to a group of glands which extend along the common and hepatic ducts, and one or two glands which are located on the cystic duct. These glands can be palpated normally with one's finger through the foramen of Winslow; but they may become quite large through infection, and since they drain the lymph from the duodenum, from the head of the pancreas, and from the gall-bladder, disease of any of these three structures will greatly enlarge same; and a blocking of these lymphatics from infection to produce symptoms will necessarily affect these glands. A duodenal ulcer or pancreatitis will do the same; and a blocking of these lymphatics from infection of the gall-bladder may cause a dense interlobular lymph-edema of the head of the pancreas associated with it. The pancreas has several sources of blood-supply and also lymphatic return, but the head of the pancreas is drained by this group of lymphatics. In the majority of cases, if enlarged glands are found, one or the other of these structures will show disease. Only rarely is there exception to this rule; and

this occurs when there is a general swelling of the mesenteric glands through malignancy or gross infection. This group of glands may be compared to those of the axilla, since, when the axillary glands are enlarged, we at once examine the arm, the breast, and local area for the source of infection. The surgeon should examine this area of the abdomen at every opportunity, that he may inform himself of the average size of the glands in both normal and diseased conditions.

When a definite source of infection is known, the treatment of mild cases of cholecystitis primarily may be medical; and for the same cases, when found surgically, prolonged drainage should rarely be instituted rather than the removal of the gall-bladder. The majority of cases are undoubtedly best relieved by cholecystectomy. Drainage alone for cholecystitis usually gives but temporary relief. This is accomplished when desired, not by inverting the fundus of the gall-bladder, but by attaching it to the peritoneal incision. Drainage ceases after a time by closure of the fistula. When the fundus is inverted, the drainage period is short, the gall-bladder healing first because of its peritoneal covering.

Many patients with cholecystitis, gall-stones, and diseases of the gall-ducts, fail to obtain complete relief of symptoms following the primary operation; and a secondary operation often shows adhesions of the pylorus or first portion of the duodenum to the liver with consequent fixation and angulation. The fatty round ligament with the gastrocolic omentum may be utilized surgically in constructing an apron of fatty tissues, which is attached near the cystic duct, and which effectually separates the stomach and duodenum from the liver. Should adhesions occur, the fatty tissues between them permit of extreme pliability.

Dr. Daniel N. Eisendrath (Chicago, Ill.): Dr. Mayo has brought out one exceedingly valuable point, and that is the necessity of removing the gall-bladder if we wish to cure these cases of chronic cholecystitis. What he said about seeing patients after operation, and feeling that we have done our duty in removing the stones alone at the primary operation, no longer holds true. The reason is, that we have pathologic changes in the mucous membrane which will not restore themselves to normal. More often than we have we must remove the gall-bladder; otherwise we leave favorable conditions which will tend toward the reformation of calculi or keep up the infection.

Anyone who is interested in this subject can find these points beautifully illustrated in the book of Ashoff and Bacmeister, who have given the reasons why we get such poor results and so often have a recurrence of calculi in cases of chronic cholecystitis. They point out the fact that the glands of Luscaka, which pass through the gall-bladder wall and go down as deep as the serosa, are frequently the seat of the deposits of cholesterin in the gall-bladder wall itself, which tend to the re-formation of calculi.

In the next place we should study the gall-bladder *in situ,* and that has influenced me, for instance, in a case where I find gall-stones in the gall-bladder, or even where I do not find them, in making the decision as to whether or not to remove the gall-bladder. In other words, my principal indication for cholecystectomy is a gall-bladder which has lost its power of contractility. It is necessary to have some knowledge of living pathology to decide that a gall-bladder which has a serous coat tremendously thickened, and whose wall is stiff and fibrous, is unable to perform the function of expelling its bile. Such a gall-bladder should be taken out, because the patient will have the symptoms referred to by Dr. Mayo, namely, the recurrence of pains and the colics present before operation. The simple removal of the gall-stones does not always get rid of the underlying pathological conditions which favored the formation of the calculi.

I think it was Kehr, of Berlin, who was the first to insist upon the necessity of examining the common duct as a routine measure, in addition to doing the routine examination of the gall-bladder. I did not believe much in his views at first, as I thought it was performing unnecessary surgery. Kehr, in one of his articles, stated that out of 54 cases of stones in the

common duct, there were 32 in which the common duct was negative to palpation.

About two years ago I began to follow this suggestion of Kehr as a routine measure in every case of chronic cholecystitis, and I now expose the common duct thoroughly in every case, and this can be readily done if one employs the Kehr incision. I expose the common duct as a routine measure and do not content myself with palpating the common duct, but open it, being guided by the three indications which Kehr laid down for opening the common duct: first, if I find small calculi in the gall-bladder; second, if there is chronic pancreatitis; third, if the common duct wall is thickened or dilated. I have followed these indications, and in a number of cases I have found when the duct was absolutely negative, so far as I could palpate it, there were a number of good-sized calculi located near the ampulla or in the hepatic duct, which I could not feel. These chronic cholangeitis cases are always benefited by drainage of the hepatic duct, even if there are no calculi.

The next point is the question of the technic of cholecystectomy. If you wish to read an instructive article on this subject I would call your attention to the recent book by Kehr, who states that five years ago he did not know the importance of anomalies of the cystic artery and hepatic duct, but now through some recent work of Ruge and others, we must be awake to the necessity of knowing why the cystic and hepatic ducts do not unite to form the common duct, as usually shown in the anatomies. Under normal conditions the hepatic and cystic ducts unite at an acute angle. But in a large proportion of cases the cystic duct and hepatic duct may run parallel to each other to a point where they both pass behind the pancreas to form the common duct. That is something that Kehr himself did not know until recently.

Then there are cases where the cystic duct comes around and forms a spiral around the hepatic duct, and, again, there are cases in which there is an accessory cystic duct entering the hepatic duct. In regard to anomalies of the cystic artery in has been found that the cystic artery ought to arise from the hepatic artery, and pass to the top of the gall-bladder. There are, however, anomalies of the cystic artery that we do not know about. We are apt to get a large cystic artery close to the common duct, and we may do damage to the common duct in ligating it.

Another point in connection with cholecystectomy is, that

it is of no benefit to take away the gall-bladder alone. Kehr has emphasized that point. It is necessary to take away, not only the gall-bladder itself, but the cystic duct down to the point where it empties into the common duct, and for that reason it is essential to know that the above anomalies occur. In these cases we, first of all, expose the neck of the gall-bladder, and take off the cystic duct as close as we can to the common duct, and insert a drain through the stump, or, better, a fish-tail drain to drain the hepatic duct thoroughly. The more thorough we are with our work in connection with these cases, the more favorable our results will be, and we shall not have as much trouble afterwards.

Dr. Byron B. Davis (Omaha, Neb.): I have been harping on the subject of operations on the gall-bladder, and when the gall-bladder should be removed, for a good many years, so much so that sometimes I fear I make myself tiresome. At the same time, I feel that up to this time we have not obtained the results in gall-bladder surgery we should have obtained, considering the amount of work that has been done. We have too large a percentage of cases that are not cured, that go along not very much better than they were before the operation was done; and, for that reason, I feel it is one of the subjects that should be studied very carefully from every standpoint, and that our discussions in regard to the matter should be very free and thorough.

My idea is, that, when we come down to gall-bladder work and reduce it to simple terms, it is a matter of good plumbing, not plumbing with something that does not change at all, but plumbing with live tissue; and we should know something about the live tissue and how it works afterwards. It seems to me, we should consider it absolutely as a matter of good drainage when we get through. Is the bile going to be able to pass through the common duct all right? Is it going to be able to pass into the gall-bladder, and then the gall-bladder be able to empty itself? Is there enough contractility there? Are there any constrictions in the cystic duct that are causing infections? If we find, after studying the case thoroughly, the probability is that drainage is not going to be good from the gall-bladder, that the gall-bladder is going to continue to have some stagnant bile in it, that gall-bladder should come out. On the other hand, if it is a gall-bladder that is not very badly diseased, and we think it is going to continue to functionate, we should never for a moment consider the removal of the gall-bladder. A great step in advance

was made when we ceased to anchor the fundus of the gall-bladder to the abdominal wall. That retarded and prevented the restoration to health of a great many patients.

A study of the common duct is one of the things, it seems to me, which should be entered into more systematically than it has been done in the past. I am glad to see that this is being done.

I recently had a case in which there was a distinct fibrous stenosis of the common duct for an inch, so that there was absolutely no chance to get a probe through. This man was intensely jaundiced for some time. In that case we resorted to the measure of doing a cholecystoduodenostomy, attaching the fundus of the gall-bladder to the duodenum; and we got a rapid restoration of health in this individual. He seems now absolutely well. In these cases we must use our heads and be guided by the pathology we find.

The subject is one we should frequently discuss, and each case should be made a mechanical problem; and we should consider that we are not doing our duty unless we leave the plumbing absolutely perfect when we get through.

Dr. Mayo (closing): Some interesting points have been advanced by Dr. Eisendrath and Dr. Davis. I think all of us are removing a larger number of gall-bladders than formerly. When we encounter a case in which the gall-bladder is very insufficiently performing its functions, we consider it best to remove it because of the possibility of further trouble. Personally, I prefer the Küttner incision for getting at the common duct.

The gall-bladder is a peculiar organ, composed mostly of elastic-muscle fibers. It probably has much to do with maintaining equalization of pressure. The peculiar method Nature has adopted to introduce the common duct into the intestine shows how little likelihood there is that backward pressure will develop infection. Three-quarters of an inch of the common duct passes through the outer wall of the bowel, and then courses between the mucous lining and muscularis. When there is gas or stasis, the tube is blocked by compression of it between the mucosa and the muscularis. The gall-bladder is attached to the under surface of the liver. The cystic duct is continuous with the pelvis of the former and, coursing S shaped, joins the common duct. Any change that produces traction on the gall-bladder will not produce a kink of the common duct.

So far as taking out the cystic duct is concerned, the worst cases we see are those patients who have had cholecystectomy, and the surgeon has injured the common duct by removing the cystic duct at a point too low.

REPAIR OF THE COMMON DUCT BY MEANS OF TRANSPLANTED FASCIA

By Carl B. Davis, M. D., and Dean D. Lewis, M. D.

CHICAGO, ILLINOIS

Following partial destruction of the common bile-duct, temporary continuity of the passage of bile has been attempted in a variety of ways. In those cases where the remnant of the choledochus cannot be implanted into the stomach or duodenum, two methods of repair have been used: the use of a foreign body, such as a rubber tube, and various forms of plastic use of the tissues of the body.

Sullivan, in 1909, successfully used a rubber drainage-tube as a link between the hepatic duct and the lumen of the duodenum. His work was done on dogs. About the same time Wilms and Brewer reported the success of the same procedure in the human. Since that time numerous similar cases have been reported. In most of the reports the tube has passed into the digestive canal, and has been expelled. In other cases it has been necessary to reopen the abdomen to remove or readjust the tube. Cahen recently reported a case where the tube did not pass, and the autopsy showed an enormous liver abscess.

Enderlen has repaired defects in the gall-bladder with omentum. Baldassari and Gardini used flaps of serosa and muscularis for the same purpose. Kehr reports the use of detached flaps from the stomach to repair the choledochus. Kausch and Stubenrauch have

constructed tubes, using all the layers of the jejunum and stomach, and anastomosing with the permanent biliary fistula already established.

The successful use of transplanted fascia in reinforcing the gastro-intestinal and urinary tracts led us to believe that fascia might be of use in surgery of the biliary system.

In our work we have transplanted free fascia from the abdominal wall to bridge gaps in the common duct and to patch partially severed ducts. In all cases the gastrohepatic omentum was destroyed in so far as any support was given to the choledochus.

In one series of cases the common duct was cut across, and the ends allowed to retract until they were one-half to one inch apart. A strip of fascia was removed from over the rectus muscle, passed beneath the ends of the severed duct, fastened by silk sutures to the proximal portion of the common duct, and passed well up on the cystic and hepatic ducts. The repair at the distal end was done by fixing the fascia to the duodenum behind the entrance of the distal end of the duct into the gut. The fascia was then folded over to form a tube, and two continuous sutures were used to make the caliber as water-tight as possible; over this the omentum was wrapped as an extra precaution.

In the second series of cases the common duct was cut partially across, and then slit lengthwise for one-half inch. A strip of abdominal fascia was placed around the duct, and the abdomen closed without drainage.

In 11 cases it was necessary to close the abdomen without drainage, as the animals pulled out tubes and gauze in the first twenty-four hours. In some cases we tried to divert the bile from the field of repair by draining the gall-bladder, but lost the animals by extravasation of bile into the abdominal cavity after

the tubes had been removed by the dogs. Wherever a leak occurred in the region of the flap, the experiment failed. This occurred in three cases, two in the first series and one where the duct was merely slit up. Five dogs were lost from infection of the abdominal wall.

Three dogs lived two months in the first series. Two of them when killed showed a patent common duct, and stools of normal color. The third dog, which lived sixty-five days, had a constriction at the duodenal end. The bile-ducts were distended to an enormous degree. The fascia was alive, and as resistant as the normal tissues. In one animal where the bile-duct had been slit longitudinally, the fascia was sutured too tight at the duodenal end, resulting in marked distention of the gall-tracts. In this case the fascia was alive, and strong enough to retain the bile.

This paper is offered as a preliminary report until animals have been carried through a longer period, and a complete histological study has been made.

DISCUSSION

Dr. Arthur T. Mann (Minneapolis, Minn.): This is too interesting a subject to let go by without some discussion. I have been much interested in this question, and have a successful case of reconstruction of the common duct in the human. The patient is a young woman, twenty-six years of age, whom I showed at a meeting of the Minnesota State Medical Association last October (1913).

The work that has been done by Drs. Davis and Lewis is very interesting, and shows that fascia will live in the presence of bile. That, I think, is a step forward. A suggestion came to me as I worked on my case in an emergency. I did not know this had to be done. The common duct was obliterated from the sepsis at a preceding operation, and something had to be done. In this case I took a rubber tube, three-sixteenths of this had to be done. The common duct was obliterated from the sepsis at a preceding operation, and something had to be done. In this case I took a rubber tube, three-sixteenths of an inch in diameter, inserted one end into an opening in the duodenum, the duodenum having been mobilized, and inverted the duodenal wall with three circular stitches, as is done in the Senn-Kader operation for gastrostomy, with the hope that

we might make a papilla large enough to act as a valve to close off the back flow from the duodenum during peristalsis. With each peristaltic wave there is intraduodenal pressure, which must force the contents up through the open tube. It is essential to make some form of valve. This can be done by making a papilla as was done in my case or by using a slit between the mucous membrane and the rest of the wall or by a Witzel operation, which folds in the wall. The duodenum in the human is not far away from the common and hepatic ducts, and as a rule can be mobilized, so that it can be brought up to these ducts, so that our problem in the human is to make a channel which will replace the common bile-duct. It can be done by an anastomosis between the hepatic duct and the duodenum. The rubber tube, I think, is a very good aid in making this anastomosis. It allows the upper end of the tube to be placed in the hepatic duct, and the duodenum can be mobilized and fastened with a few stitches without the careful suturing that is necessary for direct anastomosis. That puts things in place. The fascia can be sewed around it. The omentum can be used around it, and the tissues that are already there can be slit on both sides and partially mobilized, not detached from the blood supply, and folded over the place and stitched into place.

Dr. E. S. Judd (Rochester, Minn.): In this connection I would like to mention one case which came under my observation. The man had been operated on some ten or twelve times for gall-bladder trouble, and an attempt made to reconstruct the common duct. The condition in this case was complicated by a fecal fistula, all the feces and bile coming out of the fistula. Knowing of the work of Drs. Davis and Lewis along this line, I contemplated making a duct out of the fascia; but in the presence of infection in the abdominal wall I thought it would be safer to use a tube, and so, after closing the fecal fistula, I made a common duct after the manner of the Sullivan-Wilms method. I understand that Wilms has had to re-operate one or two cases at the end of two years on account of stricture, and that he had replaced the rubber tube. It seems to me the use of this fascia method will do away with some of these secondary operations.

Dr. Davis (closing): The point in regard to infection is well taken. We had infection in some of our cases and the fascia melted away almost at once. Although we have not tried it in infected gall-tracts, we think the flap of fascia would be lost.

THE DUODENUM: MOBILIZATION, TRAUMATIC RUPTURE, AND TOXEMIA

A CONSIDERATION OF MOBILIZATION OF THE THIRD PORTION OF THE DUODENUM, WITH REMARKS ON RUPTURE OF THIS PART AND DUODENAL TOXEMIA

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I MOBILIZATION OF THE DUODENUM

The ease with which the third, or inferior, horizontal portion of the duodenum can be exposed and mobilized is not, I believe, generally appreciated. As a consequence, the greater proportion of ruptures of this portion of the duodenum are overlooked at operation and when found are often inadequately treated, as is shown by various statistics. Thus Meerwein,[1] who reports sixty-four cases, notes that of the twenty-eight cases operated upon, in six the rupture was not found until post-mortem. In a collection of 24 cases from various London hospitals by Berry and Giuseppi,[2] 13 had been operated upon, and the rupture discovered in 7 only, while in 6 the rupture had been overlooked to be found at post-mortem. Furthermore, a review of these cases shows that practically all cases discovered were intraperitoneal ruptures. Most of the extraperitoneal ones were overlooked at operation, an error which would have been avoided by routine examination and exposure, if necessary, of the third portion of the duodenum.

The necessity for this is emphasized when we find that duodenal rupture is much more common than ordinarily supposed. Thus Gage reported 10 cases in 78 intestinal ruptures, Hertle,[3] 15 in 86, and Berry and Giuseppi, 24 in 132. Therefore, one should urge most careful observation of the duodenum in all traumatic cases. The transverse colon and omentum should be turned up so as to give a perfect exposure of the root of the mesentery and the retroperitoneal duodenum.

Two cases of extraperitoneal rupture of the duodenum have come to my service. In the first of these I attempted exposure by Kocher's method. The operation was difficult and unsatisfactory. In the second I fell upon the method described below, and was surprised at the simplicity of the technic and the excellence of the exposure.

It is well known that Kocher[4] suggested a method of mobilizing the duodenum in which the peritoneum covering the right kidney is incised vertically about an inch outside of the second portion of the duodenum. This is then detached inwards with the head of the pancreas. Hertle recognized the futility of this procedure in ruptures of the third portion, and also condemned Dambrin's[5] suggestion to go between the root of the mesentery and the mesocolon, and suggested that we should lengthen Kocher's incision along the outside of the hepatic flexure of the colon, and turn it up with the duodenum. This is a procedure that is surely possible, but rather extensive. In my second case, owing to the presence of fat necrosis in the retroperitoneal tissue, I suspected an injury of the pancreas or probably a rupture of the duodenum, because the experimental work of Richter[6] had demonstrated that a simple perforation of the duodenum could give rise to such a change. The region of the pancreas showed no involvement. I therefore turned the transverse

colon and omentum upwards, and elevated the back by means of a kidney elevator. This brought into the field the transverse duodenum, the hepatic flexure of the colon, and the root of the mesentery. Incision of the peritoneum between the reflected hepatic colon and the duodenum freely exposed the third portion of the duodenum. By tearing the peritoneum the entire retroperitoneal duodenum was freed and brought into the field from the entrance of the bile-ducts to the jejunal junction without hemorrhage or injury to any structure. The exposure is so free that it is possible a stone could be removed from the duodenal portion of the duct. The only objection to its use is the probability of retroperitoneal infection. That one can suture a considerable tear in the gut, I can bear personal witness.

On looking up the literature I find that Dambrin, in a Paris thesis, suggested on anatómical grounds the possibility of going between the root of the mesentery and the mesocolon, although Hertle later decided it was impracticable. Winiwarter[7] fell upon almost the same procedure as myself in treating a case during the last year. I did not have the benefit of it, however, as it was not published until after my own experience.

II SYMPTOMATOLOGY, DIAGNOSIS, AND TREATMENT OF RUPTURE OF THE DUODENUM

It is unnecessary here to enter into a complete discussion of the symptomatology, diagnosis, and treatment of rupture of the gut; but it may be in place to emphasize some of the salient features pertaining to the rupture of the duodenum.

A study of the combined series of cases of intestinal rupture of Berry and Giuseppi and of Hertle, 132 and 136, respectively, should give a fair example of the

various etiological factors, since they are collected from city hospitals on the one hand and from the literature on the other.

CAUSES OF TRAUMATIC RUPTURE

Blow	70
Run over	61
Kick	56
Squeeze	33
Fall	31
Miscellaneous	17
Total	268

Thus we see that approximately 70 per cent come from blows, run-overs, and kicks.

One should formulate a general rule that rupture is to be suspected in any injury in which there is a violent impact of a hard substance against an abdominal wall which occurs so suddenly that the muscles are caught unawares, as it were, and have not time to contract and protect the abdominal contents. The duodenum, though but a few inches in length, because of its fixed position in front of a bony promontory, is especially liable to injury. Berry and Giuseppi, 20 per cent of cases, Hertle, 13 per cent.

Symptoms and Diagnosis.—While the diagnosis is often difficult, in a majority of cases it can be made if certain facts are borne in mind. The diagnosis will stand or fall, generally, on five factors: first, the history of a severe sudden blow upon the abdomen; second, abdominal rigidity; third, severe pain; fourth, local tenderness; fifth, insult vomiting. These are mentioned in the order of their importance.

Without the presence of sectional or general rigidity, we must be slow to make the diagnosis except in retroperitoneal ruptures, and it is here particularly that the duodenum comes into question. It is the most

important and valuable sign. Almost all of the patients complain of severe and continued pain. The histories again and again note the pain as "excessive," "severe," or "intense."

Localized tenderness is also of great value, particularly in retroperitoneal-duodenal rupture. Nausea and vomiting shortly after the injury, which I have elsewhere designated as "insult vomiting," is also a valuable sign.

I have particularly excluded shock from the essential signs, for it is frequently absent, and its presence aids us little in arriving at an opinion. The vomiting, distention, and other signs of a subsequent peritonitis are also omitted. Berry and Giuseppi have classified the symptoms in 59 of their cases as follows:

	Present	Absent
Severe pain	51	..
Vomiting	43	2
Shock	28	6
Local tenderness	35	2
Rigidity	50	4
Distention	11	14
Added dulness	18	14
Rising pulse	37	6

Intraperitoneal rupture of the duodenum shows the symptoms above enumerated plus the signs of a rapidly developing toxemia, a rapidly rising pulse, often with an active delirium. Extraperitoneal rupture lacks at times the abdominal rigidity; but the tenderness here is localized, the pain and distress are great, and, if operation is delayed, a rapidly developing toxemia is present and vomiting is more constant.

Treatment.—Experience and judgment both teach that an immediate laparotomy is indicated in all cases where there is any probability of rupture; and in the ordinary case the general principles of intestinal surgery should be followed. In rupture of the duodenum,

however, there are several factors to be considered: first, the possibility of overlooking the rupture; second, the possibility of gangrene of the loop; third, the probability of retroperitoneal infection; fourth, the fact that the sutured bowel has no peritoneum covering it; fifth, the probability of toxemia from duodenal and pancreatic secretions.

The mortality in these cases has been much higher than in rupture of any other part of the gut. In the statistics of all types the mortality of Berry and Giuseppi was 87.2 per cent; of Hertle, 76.8 per cent; of Tavaststjerna, 76.4 per cent, while in the cases (94) of ruptured duodenum I have been able to collect there have been only seven recoveries, although Moynihan's case, which lived 104 days and died of an obstruction due to a Murphy button, should be in the recoveries. On the other hand, in Meerwein's case only the serosa was torn. These cases were collected from literature and so were exceptional cases. Of the 24 consecutive cases reported from the London hospitals, there was no recovery.

There is reason to hope, however, that if the surgeon is alive to the possibility of the injury, and searches for it in the manner suggested, the mortality may be materially lessened.

Lesions of the first and second parts may be exposed by Kocher's method; and those of the third part can easily be treated by the method of mobilization I have described above.

What should be done after the rent is exposed? Is simple suture sufficient, or should we resect and do a duodenojejunostomy, or this with a gastrojejunostomy? A study of the reported recoveries should be of value. They are as follows:

1. Herczel found a rupture at the junction of the duodenum and jejunum. He sutured the rent after four days.

2. Meerwein found an incomplete rupture, the serosa alone being torn. There was a large retroperitoneal hematoma. He resected from the middle of the descending part to the flexure, and did a posterior gastro-enterostomy and a duodeno-jejunostomy 60 cm. from the gastrojejunostomy, operating seven hours after injury.

3. Steinthal found the duodenum torn from the posterior wall and from the jejunum. He operated eight and one-half hours after the injury, sutured the bowel, did a gastro-jejunostomy, and drained posteriorly.

4. Dumont found a tear at the duodenojejunal flexure. He did an oblique suture, and drained anteriorly. The operation was done thirty-two hours after injury.

5. Wolf[8] found a tear at the junction of the horizontal and ascending parts. He sutured the tear. The operation was one and one-half hours after injury.

6. Winiwarter found a tear in the third part, sutured, and drained anteriorly nine hours after the injury.

7. Ropke[9] found a tear in the third part. He sutured the proximal end into the jejunum, and the distal end into the stomach, made a cecal fistula to flush the bowel, and closed the abdomen two hours after the injury.

From these we may argue that if the tear is not extensive we may be justified in a suture even though the rupture is extraperitoneal but that in bad cases a gastrojejunostomy and a duodenojejunostomy are indicated. Drainage may be either anterior or posterior as the case may demand.

DUODENAL TOXEMIA

On reviewing the case-histories of the two patients who came under my observation, I was struck with the similarity of the clinical course following the operation. The fact that there was something different from the ordinary type of death from peritonitis was impressed on me in both cases; and I am convinced that death was due more to some type of chemical or physical toxemia than to bacterial poisoning. I present the histories of the patients with the notes made on the history-sheets. This was especially emphasized

in the second case, in which I find the following statement made evidently as a summary before the history was filed:

"The appearance of the patient was almost suggestive of delirium tremens or more of an active toxemia. There were no positive signs of peritonitis, no vomiting, and the bowels moved freely. The patient presented the appearance of death from toxemia other than bacterial."

The well-known observations and studies of Maury,[10] Matthews,[11] and others give emphatic support to this assumption. Whether the toxin comes from the secretion of the mucous membrane of the duodenum, or from the bile, which has not been rendered harmless by dilution and colloidal suspension, or from the pancreatic juice, may be open to discussion. In relation to possible death from pancreatic secretion, in only two cases, one of my own and one other, have I been able to find a fat necrosis. Wilms has drawn attention to the fact that high intestinal obstruction produces death with extreme rapidity; and he suggests that the patients succumb to toxic absorption resulting from decomposition of stomach and intestinal contents, an assumption which, it would seem to me, lacks clinical or experimental support. That some factor other than infection is a contributive cause of death, my two cases have led me to believe.

I append a brief résumé of two cases observed in my experience at Wesley Hospital.

CASES

Case 32,945.—J. G., admitted to hospital July 14, 1912. Fifty-two hours before entering the hospital, the patient was kicked by a horse. The blow struck him in the median line

just below the umbilicus. He fell and could not get up, but was conscious. He was taken home in a buggy, and vomited two hours after being taken home. At first the vomitus was of stomach-contents. He says he took some black medicine, and threw it up, giving a black vomitus. There was no other history that would lead us to believe there was blood in the vomitus. Vomiting continued, accompanied by nausea, and soon became fecal in character. The patient complained that the stools were black. (This might have been due to the medicine taken.) Pain is constant, and does not radiate. There is tenderness all over the abdomen, most marked above. Temperature, 102.8°; respirations, 40; pulse, 120; white blood count, 20,700, excess of polymorphonuclear in type.

Immediate operation was performed, and a perforation four inches below the pylorus was found. There was laceration of the mesentery, and free blood-stained fluid in the abdomen. The mesentery was sutured, examination of the abdomen made, and the duodenal rupture sutured with much difficulty. Kocher's method of exposing the duodenum was used; but, owing to adhesions present from a previous infection of some type, the operation was difficult and the result unsatisfactory. The abdomen was closed in the usual manner with drainage. The patient died six hours after the operation.

Post-mortem examination disclosed peritonitis about the wound in the gut only. There was no general peritonitis, but a violent injection of the peritoneum, and the subperitoneal tissue was very edematous. The history notes that the patient was not an alcoholic.

Case 41,477.—F. S., admitted to hospital thirty-four hours after injury. He was struck on the anterior abdominal wall, in the epigastric region, by a wagon pole and crushed between the wagon pole and a telegraph pole. He vomited shortly after the injury, and then again twelve hours after he received the injury, after which time he vomited repeatedly. He complained of severe pain in the epigastrium, but continued at work until near noon. He slept little that night, and entered the hospital the next afternoon at five o'clock. There was marked abdominal distention, with tenderness especially marked in the right inguinal region, but also in the left inguinal region and epigastrium. There were no external signs of violence; pulse, 126; respiration, 32; temperature, 97°.

Operation.—Gas-ether anesthesia was given. On opening the abdomen the intestines were seen to be injected as by a violent irritant. There was a thin faintly bloody serum, not purulent

nor apparently infectious. A plaque of fat necrosis was noted on the hepatic flexture of the colon below the cecum. The entire retroperitoneal tissue, including the pelvic connective tissue, seemed to be doughy, as if filled with serum; and a considerable tumefaction was noted in the right side of the spinal column behind the peritoneum.

Diagnosis of extraperitoneal rupture of the duodenum was made. A peritoneal incision was made between the duodenum and colon. This exposed the duodenum freely, in which could be seen a longitudinal tear about an inch and a half in length on a level with the common duct and below. The retroperitoneal mass contained bile and intestinal contents, which had extravasated down to the right inguinal region, thus explaining the tenderness noted there. The wound in the gut was sutured with two layers of linen. Drainage was carried out posteriorly underneath the colon below the costal margin by a tube leading to the duodenum and a cigarette-drain leading into the right inguinal region, both being under the colon and under the peritoneum. A rubber tube with gauze inserted was carried out at the lower angle of the abdomen. The wound was closed by figure-of-8 sutures.

Following the operation the patient was given large amounts of normal salt both subcutaneously and by rectum. His pulse rapidly rose from 126 to 160, respiration from 32 to 44, and temperature to 103°. It is noted on the history sheet that "patient was placed in a restraining jacket," "unable to give continuous tap-water by rectum on account of restlessness of patient," "patient delirious," etc.

On the day after the operation his bowels began to move; he did not vomit; and at the end of fifty hours he died. Post-mortem examination disclosed no peritonitis. The following note is on the history sheet: "The patient became very talkative and very restless, almost suggestive of delirium tremens, yet more of an active toxemia. There were no positive signs of peritonitis, no vomiting, and the bowels moved freely. The patient presented the appearance of death from toxemia other than bacterial. He was not an alcoholic. Death, cardiac."

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DISCUSSION

Dr. Warren A. Dennis (St. Paul, Minn.): This has been an extremely interesting paper, especially so to me because I recently had a case of this kind which falls under the class Dr. Kanavel has reported, and was not recognized. The man entered the hospital at about noon one day after I had been around, and did not seem seriously injured. The internes and my associates looked him over, and decided there was a large nervous element in the case. He was not seen until the following morning by me. It was very evident at this time that the man was sick. His pain was excruciating. There was some distention of the abdomen and marked tenderness, and he was vomiting. He was taken to the operating-room at once, and the abdomen opened. I found a rather dark-colored serum. I looked over the intestine carefully, but could find only two places where it showed any irritation. The bowel here presented the appearance of contusion with some coagulated fibrin on the surface. There was no fecal matter in the abdominal cavity whatever. I closed him up, feeling there was such a thing as an abdominal contusion, with perforation of the bowel by toxines or bacteria without actual gross rupture. The man died that night about 1 o'clock. Autopsy disclosed the fact that he had a complete rupture of the duodenum in its terminal portion at its junction with the

jejunum, and the whole retroperitoneal space was filled with fecal matter. It was something entirely new in my own experience; and therefore I have been extremely interested in Dr. Kanavel's paper on this account, and the possibility that is open in these cases provided they are recognized early.

I have not had the time to look up the literature on these cases, but it would seem to me the plan of doing gastroenterostomy or a duodeno-ileostomy, or a jejunostomy would be more desirable than to attempt to suture the ends of the bowel, for the reason that it would give some opportunity to mobilize the torn ends and perhaps get a complete serous covering for these ends in that way.

Dr. Charles H. Mayo (Rochester, Minn.): I should like to ask the author of the paper if in looking up the literature on the subject there was any reference to the time after meals in which the injury occurred, or if it had to do with the question of rupture?

Dr. R. C. Coffey (Portland, Oregon): I have had two cases of this kind with 100 per cent mortality, and I think it is a condition which is more frequent than we have heretofore thought or believed. In fact, I think it is one of the most frequent traumatic subcutaneous ruptures in the abdomen, and is purely for anatomical reasons, where the spinal column crosses the bowels. In the cases I have seen the duodenum has been crushed to a pulp. Both of them have been due to the running over of a heavy wagon across the abdomen. That is probably the most frequent way in which the injury occurs, notwithstanding the fact that the statistics given by Dr. Kanavel show that blows are a more frequent cause of the injury. I think in every instance in a child, where the small muscles are not able to resist a heavy wagon, there should be an exploration made, as these cases, as a rule, do not give many symptoms until it is too late to operate. I have fully made up my mind what I shall do with the next case that presents itself, namely, mobilize the duodenum to a certain extent above the colon, and make an end-to-side duodeno-jejunostomy, by cutting off the duodenum below the ducts and bringing up a loop of jejunum for anastomosis, and then pack the retroperitoneal space with gauze. We find in cases of rupture of the pancreas, if we pack with gauze, the great majority of cases get well, but, if we do not open up the space and pack it thoroughly, they will not get well.

Dr. W. W. Grant (Denver, Colo.): Dr. Mayo's question is certainly significant, for all surgeons recognize the fact that with any blow or injury to the abdomen, if the intestines are full of gas or fecal contents, or undigested material, the extravasation is much greater, and consequently results are much graver.

There is another point, regardless of whatever method you may use to close the rent in the intestine, which has something to do with the mortality, which has been indicated by a few remarks of the author of this paper. The point is the question of duodenal toxemia. A recent review of the experiments and investigations that are being made shows that there is a form of toxemia from the duodenum in injury and in obstructions which has not been fully understood or explained, but which has a great deal to do with the mortality. The question has come up in reference to the extremely greater mortality in high obstruction of the intestine or jejunum. These cases die much more frequently, the condition is much more dangerous than any obstruction of the lower parts of the small intestine, and it is believed now that the mortality is due to a yet unknown toxemia from the duodenal-mucosa or the submucous portions of the duodenum itself, and that probably has more to do with the mortality and results than any varied or elaborate form of technic in closing the intestine.

Dr. Kanavel (closing): In answer to Dr. Mayo, I will say that I have not been able to find case-histories concerning the relation of time of taking food to the injury.

PSEUDOPANCREATIC CYSTS, WITH A REPORT OF FOUR CASES

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In discussing pseudopancreatic cysts, one is tempted to take issue with the term as applied to the existing pathologic condition. The condition is essentially a collection of fluid in the lesser peritoneal cavity, and not a true cyst, and resembles a pancreatic cyst only in that the fluid sometimes contains the pancreatic ferments. The old term of traumatic cyst of the pancreas, as first used by Küklenkampff and Senn, is of course erroneous; for it is in no sense a true cyst, nor is it directly connected with the pancreas. A truer and more descriptive term would be "an accumulation of fluid in the lesser peritoneal cavity as a result of trauma to the pancreas."

Küklenkampff's case, reported in 1882, is the first case observed and reported in the literature; and this was considered a true cyst of the pancreas. This was followed by Senn's classic paper, with a report of a case, which appeared in 1885. Senn recognized that, because of its slight protection and its rather exposed location when the stomach and colon are empty, the pancreas is susceptible to injury. He was, however, unable to find in the literature any allusion to stricture of the duct as a result of trauma. He stated that "the clinical history of several rapidly growing cysts tends to prove that obstruction of the duct occurred in this manner."

A careful survey of the history of Senn's case convinces one that he was dealing with a collection of fluid in the lesser peritoneal sac, and not with a true cyst. He assumed that the condition was a retention cyst caused by a trauma to the duct, with subsequent fibrosis and occlusion. He attempted to produce the condition by animal experimentation, and failed. He explained the blood in the fluid in several possible ways. Fenger, in 1887, and Steele, in 1889, reported cases as true cysts of the pancreas following injury, both of which were probably pseudocysts.

Jordan Lloyd was probably the first to recognize and describe the true pathology of this condition. He reported two cases, one of which was fatal, and was subjected to necropsy. The other patient was operated on, and the exact pathologic anatomy noted. In both of his cases the fluid contained pancreatic ferments.

In 1895 Leith collected seventeen cases from the literature, including the ones reported by Küklenkampff, Senn, Kuester, Fenger, Steele, and Richardson. Many of these seventeen cases were considered true cysts, but Leith believed them to be pseudocysts.

A brief review of the anatomy of the lesser peritoneal cavity shows that it varies in size within rather wide limits. It lies below and behind the stomach, the gastrohepatic omentum, and the anterior layer of the great omentum. It is directly in front of the pancreas; and the peritoneum is closely adherent to this organ at this point, not being separated from it by any considerable layer of fat, even in fleshy people. This is to be noted as an essential factor in a consideration of the pathogenesis of pseudocysts following trauma.

The lower boundary of the sac is formed by the upper layer of the transverse mesocolon. Laterally, it extends from the hepatic to the splenic flexures of the colon. The right limit is formed by the folding

of the peritoneum to form the foramen of Winslow. The left limit is the hilum of the spleen. The upper limit is formed by the diaphragm and the liver. The spigelian lobe of the liver is the only part of this organ that lies free in the cavity. The colon lies below the cavity.

Lloyd found when the cavity was distended with plaster of Paris that the greater bulk was found deposited in the hollow to the left of the spine, which would tend to explain the prominence of the tumor at this point when the sac is distended with fluid. In considering trauma as an etiologic factor in producing a distention of this lesser sac, one must recognize that an inflammation from any cause which involves this portion of the peritoneum is capable of inducing the same effect. Trauma to the abdomen, particularly crushing injuries across the upper part, can readily impinge the pancreas against the spine, and lacerate it extensively; for, if the stomach and colon are empty, this organ is not well protected.

It is now conceded that such an injury will so tear the overlying peritoneum, and crush the pancreas beneath it, so as to allow an escape of pancreatic secretion and blood into the lesser peritoneal sac, producing an inflammation which results in a plastic exudate capable of closing the foramen of Winslow. With a continuation of this process a distention of the cavity is inevitable. It has been thought that fluid might collect between the peritoneum and pancreas following injury, but this does not seem probable. A severe trauma was inflicted on the upper abdomen in each of the four cases here reported.

Fat necrosis has been observed in many of the reported cases, which resulted from an escape of pancreatic secretion through the foramen of Winslow before it closed, or from a flow along the lymphatics. Two of the cases here reported showed fat necrosis.

The fluid found in the reported cases has shown pancreatic ferments in a considerable number of them, the fat-splitting and sugar-reducing ferments being the ones usually found. The amount of ferment found is dependent on the extent of laceration of the pancreatic tissue, and relatively on the quantity of fluid exudate within the sac, causing a dilution of the escaping pancreatic secretion. The fluid from two of our cases showed pancreatic ferments.

In considering the diagnosis of a collection of fluid in the lesser peritoneal sac due to trauma, one is impressed with the similarity in the nature of the injury that occurred in a majority of the reported cases. While it is recognized that the pancreas is a deep-seated organ and fairly well surrounded by protecting tissues, yet it rests directly on the spine; and if the stomach and colon are empty and the anterior abdominal wall thin, as it is in children, then the conditions favor an injury to the pancreas from any blow or crushing force which will thrust the gland against the firm vertebra.

Three of our four patients were children; and all of them sustained their injury by being run over by a wagon. The fourth was an adult, who had received a sudden sharp blow in the upper abdomen. He was a porter on a sleeping-car, and was thrown violently against a seat in a railroad wreck. The history is, therefore, of great value in arriving at a diagnosis.

The subsequent clinical course in all these four cases is strikingly analogous. Each case gave a history of rather severe abdominal pain, with nausea and vomiting immediately following the accident. None of the cases were seen on the first day, so the amount of shock cannot be estimated. Two of the cases were seen on the second day, and both had a temperature from 100° to 101° F., with a slight acceleration of the pulse-rate. This continued until operation. There

were some tenderness and slight muscular rigidity over the epigastrium. These symptoms continued until the appearance of the tumor, which was first observed in one case ten days after injury, and in the other twenty-three days after the accident. It is probable that in the latter case the tumor had existed a number of days before being discovered. In the other two cases it was first noticed in twelve days and three weeks, respectively, after the trauma. In practically all the recorded cases it is noted that the tumor made its appearance between ten days and four weeks subsequent to the trauma. It is our conviction that a mass can be made out in all cases in eight or ten days, if carefully searched for. The three of our four cases which occurred in children presented a smooth globular mass just to the left of and above the umbilicus. This mass protruded from beneath the left costal arch, and was firm in consistency, giving a sense of fluctuation.

Distention of the stomach and colon with gas demonstrated the mass between these two organs. There seemed to be slight movement with respiration.

The fourth patient was a colored porter, and he had received his injury about three months before coming to the Post-Graduate Hospital. He gave the history of having first noticed the "swelling" about three weeks after his accident. The tumor mass was larger, firmer, and more fixed than the other three cases, and occupied the entire epigastrium, being a little more prominent to the right of the median line. When the stomach was distended with gas it was found lying over and below the most prominent part of the mass. It seemed fixed to the anterior abdominal wall, between the umbilicus and the right costal arch. When the lesser peritoneal sac becomes distended with fluid it may approach the anterior abdominal wall between the stomach and colon, which is the usual location, or below the colon, or above the stomach, through the

gastrohepatic omentum. In three of our cases the mass was found between the stomach and colon, and in the fourth above the stomach.

In each of the four cases there was a moderate leukocytosis with a slight increase in the polymorphonuclear cells.

It is unnecessary to add that an exploratory needle was not used in any of the cases.

In the first two cases seen a positive diagnosis was not made, one of them being the case of the adult, which had existed for three months; the second two seemed so typical that we ventured a rather positive statement as to the condition.

In taking account of the evidence which makes for a diagnosis, a consideration of the manner in which the injury to the upper abdomen was inflicted is all-important. Crushing injuries, such as a wheel passing over the abdomen of a child, are probably the most frequent cause.

The appearance of a cystic tumor ten or twelve days after such an injury, protruding to the left and above the umbilicus, lying between the colon and the stomach, and accompanied by a slight amount of pain, tenderness, and elevation of temperature, makes the diagnosis of a collection of fluid in the lesser peritoneal sac most probable.

The four cases here recorded were treated by incision and drainage. In the first case, the adult with the history of three months' duration, in which the cystic mass appeared above the stomach, the sac was found adherent to the anterior abdominal wall, and was opened without entering the general peritoneal cavity. No fat necrosis was observed, but the fluid was seropurulent, and contained pancreatic ferments. It was not blood-tinged.

In the other three cases there was a protrusion between the stomach and the colon, and the free peri-

toneal cavity was first walled off with gauze, and the sac opened and drained. In two of these cases the fluid was blood-stained; and both showed a moderate amount of fat necrosis. There was no serious amount of excoriation of the skin in any of the cases; and the sinuses closed rather rapidly and without any unusual incident. All four patients made a complete recovery.

DISCUSSION

Dr. D. C. Brockman (Ottumwa, Iowa): My experience with pancreatic cysts is limited to four cases quite different from those reported by the essayist. In none of my four cases was the cyst due to trauma. Two of them were due to gall-stones. One of them was due to acute hemorrhagic pancreatitis, resulting from gall-stones. A very large cyst formed about two weeks after the attack of pancreatitis. We opened and drained the cyst, turning out a large amount of clots and serum. The patient went down steadily from 220 pounds to less than 100 pounds at the time she died, ten weeks afterwards. At the autopsy we found entire destruction of the pancreas; there was not a scrap we could prove to be pancreatic tissue by the microscope, and the gall-bladder was full of stones.

Another case was a young fellow, a livery-stable boy, in whose case there was no history of injury. We had no history pointing to the cause of the trouble. Dr. Senn proved by his experiments in 1884 that a partial closure of the pancreatic duct always resulted in pancreatic cyst. He repeatedly in his laboratory produced pancreatic cysts by partial closure of the pancreatic duct, while a completely closed duct always resulted in atrophy of the pancreas, and the death of the animal. I was closely associated with Senn at this time, but did not see the experiments. He published a very able paper at the close of 1884 in which he taught these principles.

Dr. Besley (closing): It has been our observation, and anatomists tell us, that the posterior peritoneum is firmly adherent to the pancreas and closely associated with that organ, and there is never any considerable layer of fat between the peritoneum and the pancreas even in fleshy people. This accounts for the ease with which a rupture or a tear of the pancreas may occur when it is impinged against the firm vertebra behind.

CONGENITAL PYLORIC STENOSIS

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Recent studies of congenital pyloric stenosis have shown it to be a disease of much importance. Until the past few years these cases were treated as cases of marasmus and the true pathology of the condition remained an unknown factor.

The frequency of occurrence of this condition is hard to estimate, as the statistics on this point are very meager; but observations made at the Bethesda Hospital by Dr. T. Wister White, of St. Louis, showed five cases originating in one thousand babies. This estimate of one case in every two hundred babies seems to be fairly accurate as to the frequency of the disease.

The pathology of the condition consists, primarily, of great enlargement and thickening of the pylorus, caused by hypertrophy of the circular muscle. This causes an obstruction of the pyloric opening, which may be more or less complete. The enlargement is about the size of the last joint of the thumb, is smooth, not adherent, and usually can be palpated through the abdomen. The stomach becomes very much enlarged, and the intestines small and shrivelled.

Scudder holds that the obstruction is an anatomic one, and is not dependent upon physiologic causes. Its cause is unknown, but the hereditary factor seems to be prominent, as two of our cases occurred in families where the condition had occurred before.

The age at onset of the symptoms of this disease is usually between the third and sixth week of life. The symptoms vary according to the degree of obstruction; and it seems certain that conditions exist varying from slight obstruction (the spasmodic type) to complete obstruction, where a fine probe can hardly be pushed through the pyloric opening. Usually, we have in these cases a history of a normal child until it is between three and six weeks old, when the manifestations make their appearance.

The symptoms of the disease may be considered under four heads: (1) vomiting; (2) constipation; (3) the gastric waves or contractions; (4) tumor.

The first symptom to attract attention is the vomiting. At first this is hardly sufficient to demand special attention, and may be temporarily improved with a change of food. Any improvement, however, will be temporary, and the vomiting soon becomes more aggravated than ever. The vomiting is distinctly projectile. The propulsive force is at times so great that a child of three months, lying on its side, has been seen to eject the contents of the stomach a distance of three feet. Occasionally, the child may not vomit for several minutes after taking food. In the latter stages of a severe case he ejects a part or all of every feeding.

The loss of weight is very rapid. The constipation is most marked, and, in severe cases, may be practically absolute. Mucus alone may be found in the stools. The urine is scanty, and dark in color. The appearance of the child is like that in marasmus. The face is wrinkled, and the tongue and mouth dry.

On inspection the upper part of the abdomen is found to be enlarged, and the lower part narrow and empty. The outline of the stomach can frequently be seen reaching as low as the umbilicus, and, on palpation, gives a sense of muscular resistance greater

than that of a distended colon. The muscular or gastric waves soon make their appearance, and are pathogonomic of the condition. These waves are due to peristaltic contractions of gastric muscle. These contractions may manifest themselves in a rounded eminence rising at the left gastrochondral border, where it remains a short time. Usually, however, they manifest themselves in waves passing across the epigastrium, and disappearing on the right side. Sometimes three such waves are in sight at one time. They are rarely more than one inch in height. As a rule, the waves occur after taking food, but may often be brought on by irritating the skin of the abdomen in the gastric region.

The enlarged pylorus may readily be palpated in the disease. It occurs as a smooth, rounded mass about the size of the end of the thumb.

The diagnosis in a typical case is easily made, as the presence of tumor, the gastric waves, the malignant vomiting, so to speak, and the marked constipation, make the picture clear. The presence of enteritis, and also of brain lesions in their early stages, may tend to obscure the diagnosis. In the early stages the passage of a tube and recovery of retained milk is a valuable diagnostic aid.

The treatment of congenital pyloric stenosis may be considered under two heads: (1) medical; (2) surgical.

In all our cases medical treatment had been thoroughly tried, and in several it had been persisted in so long that surgical measures offered only a long chance. Two cases coming in in the past year were considered so hopeless that after being admitted to the Hospital, the Pediatrician in charge considered it useless to seek surgical relief, and they died in a few days after admission.

The medical treatment consists in feeding by a tube. The stomach is washed out, and the food inserted through the tube. Children will often retain food given in this way in the spasmodic cases where they will eject it if swallowed. In some of these cases nutrient enemeta of whey or other foods give temporary relief.

The question of when to operate in these cases is a vital one. Scudder states that an operation is indicated in all cases where a distinct tumor is palpable, and also in those cases of spasmodic variety where, under medical conditions and treatment, conditions become very grave. Lowenberg states that, in determining whether or not to operate in these conditions, we have three factors of importance to consider: (1) weight; (2) strength; and (3) character of the constipation. He gives the following rule: If a small amount of food is retained and the weight remains stationary, or there is a slight weekly gain, or if the strength does not seem to be ebbing away, and if with this we have an occasional large bowel-movement, we are justified in waiting even if vomiting persists. If no food, or very little food, is retained, with loss of weight and increasing constipation, the patient should be operated upon before he becomes too weak.

When these cases are studied from a surgical standpoint there are several important factors to be considered. Infants stand the shock of operations badly. They are prone to infections; and the reparative power is so slight that the wounds may simply fall apart when one would expect them to be thoroughly healed. The operations recommended in these conditions are Loret's operation, or divulsion of the pylorus; gastroduodenostomy, or pyloroplasty; and gastrojejunostomy. The first two of these operations have almost been discarded as being far inferior to the posterior gastro-enterostomy.

Several points regarding the preparation and operation are worthy of note. None of the very strong antiseptics have been used in our cases, but simply thorough cleansing with soap, sterile water, and alcohol. Ether is the anesthetic of choice with babies; and a tank of oxygen should be available. The incision is best made to the left of the median line, and rather low because of the large size of the liver. The usual Mayo-Moynihan operation is done. In the infant, however, several points are to be carefully observed. The bowel is about three-eighths of an inch in diameter and is so fragile that the least undue tension will cause the stitches to tear through like tissue paper. The finest needles and suture material are to be employed. Fine chromic catgut may be used for the inner layer of sutures, but, I think, fine silk, about No. 00 in a No. 10 milliner's needle will be found the most suitable. The bowel is usually so shrunken that every care must be taken to avoid a kink. A kink caused the death of my second case.

The results obtained by treating these cases surgically were at first anything but satisfactory; but with increasing experience, and earlier recognition by the pediatrician, operative intervention before the vital powers of the child were too far exhausted, has led to some most brilliant results in this comparatively new field. One author reports that the statistics collected from 1898 to 1905 showed a mortality of 46.05 per cent. Haines, of Cincinnati, analyzed the records of 129 cases. In this series divulsion of the pylorus was done 29 times with 14 recoveries, or a mortality of 51.07 per cent; gastro-enterostomy, 76 cases with 35 recoveries, or a mortality of 53.9 per cent; pyloroplasty, 23 cases with 11 recoveries, or a mortality of 52.1 per cent; pylorectomy in one case with a fatal result. T. S. Bunte (Cleveland) reports 7 cases with 4 recoveries. All operations were performed by an-

terior gastro-enterostomy. Statistics by Scudder show that he has performed 14 of these operations with 1 death; Richter, of Chicago, reports 21 cases operated on with only 3 deaths; while Stillman, of San Francisco, reports a group of 10 cases operated upon with only 1 death.

The cases of congenital pyloric stenosis that have been referred to me for operation have all come from pediatricians of broad experience, who draw their work, not only from St. Louis, but from a large surrounding territory. I have operated upon 10 of these cases, 5 recovered and 5 died. This high mortality is due to two factors,—faulty technic in one or perhaps two of my early cases, and almost hopeless complications in the other three. In one a meningitis developed a couple of days after operation, and two had intense jaundice at the time. The jaundice cases acted peculiarly. One lived eleven days, and the other five days. Both died in the same way, i. e., from the tissues simply falling apart, as nature had not made any efforts at repair.

Case 1.—H. W. P., Jr., born Sept. 14, 1911, weight 8 lbs. The child was breast fed, and seemed to get along well until fourteen days old, when he cried and vomited after each feeding. The attending physician said it was indigestion, and had him taken off of breast milk and put on barley water, rice water and albumin water in succession with no result.

The child continued to vomit; his bowels had not moved, except with the aid of castor oil or an enema, when the movement was black and scanty, containing a great deal of mucus.

The condition gradually became worse until October 19th, when a posterior gastro-enterostomy was performed. At the time of the operation the weight was 6 lbs. and 13 oz.

The child improved immediately, and the second week gained a pound. At six months he weighed thirteen pounds, and at seven months, thirteen and three-fourths pounds. At one year his weight was 20 pounds, at 2 years 26 pounds. He seems to be as strong and active as any other child.

Case 2.—Virginia S., born November 26, 1911. Parents,

healthy. The parents have one child living at the age of four years. Two years ago a baby died of symptoms identical with those of the present case. Apparently healthy at birth, it thrived until three weeks old, when vomiting began, and death occurred when six weeks old. No diagnosis was made.

Virginia S. was normal at birth. There was no more spitting up than in an ordinary baby until December 23d, when four weeks old. December 25th the parents became alarmed at severity of vomiting. December 28th, as described by mother, "the vomiting was violent, and often was greater in quantity than the previous feeding." The baby was in poor condition: prostration, slight fever, peristalsis of stomach visible, and tumor palpable.

The child was removed to the Bethesda Hospital, and lavage was begun. The vomiting decreased, but a large proportion of food was retained in the stomach and removed by washing. The child gradually became worse; and an operation was decided upon. A posterior gastro-enterostomy was performed on December 30th. At this time the weight was 6 lbs. and 14 oz. This failed to give relief and obstruction was feared, and the abdomen was reopened in thirty-six hours. A kink was found in the jejunum owing to a misplaced stitch. The baby died on January 2d.

Case 3.—Baby O., born March 28, 1912.

The family history is uninteresting except that this is the sixth child in the family, five of whom are living. One child died ten years ago at the age of six weeks. It was a healthy baby at birth, and did well for two weeks, when it was taken with vomiting and died of inanition after four weeks' illness. This was probably a case of pyloric stenosis.

The history of the present case is, that the baby was born March 28th and weighed ten pounds. She did well for three weeks, but on April 18th was taken with severe vomiting. The bowels moved frequently, and the movements were very scant. A tumor could be palpated at the pyloric region, and muscular peristalsis was visible.

The child was breast fed until April 22nd, when Eskay's food was tried. She retained several feedings the first day, and also some the second day, April 24th. On April 25th vomiting was most persistent and weakening. On April 26th temperature was 99.5°. Dr. Saunders saw the case at this time, and started lavage and feeding with peptonized milk. In the first twenty-four hours of this treatment, 11¼ ounces were retained; in the next twenty-four hours 7½ ounces were re-

tained; after that not more than one-half ounce of each feeding was retained.

The child lost very rapidly. On April 22d, when sick four days, the weight was 10.43; on April 28th, 9.12; on April 29th, 8.5, showing a loss of weight of one pound and nine ounces in thirty-six hours. The temperature was normal from April 18th, when sickness commenced, until April 26th. It then went up to 103°, and continued to run 100°-102° until after operation.

Operation.—Posterior gastro-enterostomy was performed on April 29th at 5 P. M. The patient was treated with proctoclysis and lavage. After midnight mother's milk was given, and the child began to retain it. Following this there was rapid recovery. The child took the bottle in four days; vomiting gradually subsided; stools increased in amount; and the temperature became normal in five days, the decline being gradual. At the end of the first week it was found that the child had gained three ounces. During the second week she gained six ounces. From this on recovery was very rapid.

On December 11, 1913, the mother reports that the child is twenty months old and weighs twenty-five pounds; and further she adds, "she is exceptionally active and enjoys splendid health."

Case 4.—Chas. G. L., male child, born May 10th, weight 8¼ pounds. The child was normal except for a slight jaundice until May 26th, when the characteristic signs of pyloric stenosis developed.

The vomiting was projectile, constipation marked, gastric waves prominent, and a tumor palpable. The loss of weight was rapid. Jaundice was more marked. The patent was referred for operation. Posterior gastro-enterostomy was performed on June 1, 1912. Its weight at the time was five pounds. The child did well for a week, and then developed convulsions, and died on June 11, 1912.

Examination showed that the tissues had simply fallen apart, both at the new opening and at the abdominal wound.

Case 5.—Helen W., born June 24, 1912. The child was normal for five weeks when the characteristic symptoms of pyloric stenosis manifested themselves. Tumor palpable. Weight when admitted to the hospital, 6 pounds and 7 ounces, July 21, 1912.

Posterior gastro-enterostomy was performed. Feeding in 1 drachm doses began six hours after operation. This amount was rapidly increased.

The baby did well and was discharged cured August 10th.

Case 6.—J. E. N., born June 25, 1912; weight, 8 pounds. The child was normal until July 21st, when the characteristic vomiting, gastric waves, and constipation were manifest. A tumor could not be palpated. The loss of weight was rapid. Severe jaundice existed as a complication.

Posterior gastro-enterostomy was performed on July 28th. The weight at this time was 5 pounds. The child did badly after the operation, and died August 1st. In this case the wounds simply fell apart. This was a case of the spasmodic type, as the pylorus was very little thickened, and the opening was adequate. This falling apart of the wound characterized both of the cases in which jaundice existed, as the reparative power seemed to be absolutely lacking. I question if a young baby with a severe jaundice will survive an abdominal operation.

Case 7.—Henrietta L. C., of Fort Smith, Ark., was born July 24, 1912. At birth she weighed eight pounds. Her weight increased to 11½ pounds and child was normal until she was between four and five weeks old, when the projectile vomiting and other signs of pyloric stenosis developed. Loss of weight was gradual. She was brought to St. Louis for operation on October 4, 1912. Tumor was well marked, and emaciation was extreme. The child was then ten weeks old, and weighed five and three-quarters pounds.

Posterior gastro-enterostomy was performed. Anesthetic, ether. The child fed on mother's milk. It never vomited after the operation. Gained eleven pounds in the next twelve weeks.

On December 6, 1913, the father reports the child in perfect health and weighs twenty-four pounds.

Case 8.—N. B. was referred for operation on April 8, 1913, at the age of four months. The early history was not obtainable. The meager history given was that symptoms of pyloric stenosis had developed at the age of five weeks, and had been persistent although a tumor was not palpable. When referred for operation, the child had a running ear and a temperature of 101°. Its weight at the time was 7 pounds 15 ounces.

Posterior gastro-enterostomy was performed, and the child

did well for three days, and then developed meningitis, from which it died at the end of a week.

This case was simply one of spasm of the pylorus, and a true stenosis did not exist. Examination of the cerebrospinal fluid showed that the case was simply one of meningitis from the ear infection.

Case 9.—Herbert H., born June 15, 1913; weight, 9 pounds. This increased to eleven and one-half pounds by the first of July. On July 6th commenced to have the characteristic vomiting of pyloric stenosis. This was accompanied by the other characteristic signs, including tumor. The loss of weight was rapid. From July 8th to 11th the child lost one and one-half pounds. Conditions became gradually worse until July 26th, when the child weighed 5 pounds 8 ounces. Conditions were so grave that it was thought if a gastro-enterostomy were attempted the child would die on the table. In this case divulsion of the pylorus was done by means of forceps passed through a small opening in the stomach. The pyloric muscle could be felt to give way. Four ounces of water were inserted into the bowel by means of a catheter passed through the pylorus. Conditions improved for the next few days, and then the child became worse, and died on August 3, 1913. The abdominal wound simply fell apart, and it was found that the pylorus had contracted down again.

Case 10.—My last case is that of Mallory P., a twin, born October 11, 1913. The child was premature, and at birth weighed only 4 pounds and 2 ounces. On Friday, November 11th, the severe vomiting commenced, and the gastric waves became prominent. Constipation was almost absolute. A tumor was palpable. I operated on November 14, 1913. The child at the time was pulseless at the wrist, and had been so for more than twenty-four hours. Its weight was 3 pounds and 15 ounces. Its temperature was subnormal. Posterior gastro-enterostomy was performed. The next day and for several days following, he had several mild convulsions, and would occasionally cry out with pain, but for the most part was perfectly still with eyes partly open, and unable to swallow. All food was given through a tube, beginning with a few drachms of water almost immediately after operation (within two hours). Mother's milk was given the next day; at first one-half ounce every two hours, and increased to one ounce. One-half ounce of water with three per cent cane-sugar was given the alternate hour. Practically all vomiting ceased,

and the stools became normal. It was ten days before the child could swallow, and more than a week before it could close its eyes. Gain in weight was slow, only about three ounces the first two weeks. After this the child began to improve rapidly. It gained four and one-half ounces the third week; twelve and one-half ounces the fourth week. This is the fifth week, and the gain has been one pound for the week.

This is the smallest child of which I can find any record of having been successfully operated on for this disease.

In conclusion, I wish to add that all of the cases submitted to operation should be fed upon mother's milk. They should have the care of a nurse thoroughly trained in pediatrics, and a pediatrician should have charge of the feeding. These things are, I believe, absolutely necessary if the best results are to be obtaind.

In regard to the application of surgery to these cases: I believe I am justified in saying that, if it can be shown, first, that a tumor is present; second, that the operative treatment is attended with a mortality as low or lower than the medical treatment by lavage and diet; third, that if, with an equal or lower mortality, surgery gives immediate results, and saves parents the prolonged suspense of doubtful treatment that may demand operative relief in the end—then operative treatment is justified.

In regard to the mortality in these cases: It was high in the early series of cases in the hands of different operators. With an increased experience, however, it has come down until it is lower than medical treatment in the most skillful hands.

The second point is easily answered. In the doubtful cases treated by lavage, the outcome is in doubt for several weeks at least; the treatment is nerve-wrecking, and the outcome uncertain. Surgery, on the other hand, yields results almost instantaneous; and a child that today is crying, vomiting up all food,

blood, and the picture of marasmus, may, in a week's time, present a condition of rapid convalescence and contentment.

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FOR DISCUSSION, SEE PAGE 269

ULCER OF THE STOMACH IN CHILDREN BEFORE PUBERTY

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Round ulcer of the stomach and duodenum in children before puberty is relatively rare, although I am convinced, from recent experience and from a review of the literature of the subject, that it is a much more common lesion than most surgeons realize. I believe that many obscure abdominal symptoms in children, heretofore ascribed to appendicitis, gastro-enteritis, ptomain poisoning, and the like, are due to ulcers.

All authors whom I have consulted, save Jacobi, declare that true peptic or round ulcer of the stomach and duodenum is rare in children. The following statistics indicate the rarity of ulcers in children: Welch (reference "Pepper's System of Medicine") found one case in a child under ten in a series of 607 cases of gastric ulcer. Dusser (Paris Thesis, 1890), in a series of autopsies at a children's hospital, found 9 cases of gastric ulcer and 4 of duodenal. Wallenstein, quoted by Osler, found 5 cases of gastric ulcer in 390 autopsies at the Baby's Hospital, New York City. Koplik reports only 1 case of gastric ulcer in his experience covering 300,000 cases of diseases in children.

After recognizing a case in my own practice, and successfully operating upon it, I was stimulated to investigate the subject from the surgical point of view.

This paper is based upon data gleaned from all medical literature to which I had access, upon personal correspondence with 150 members of this Association, and with all children's hospitals in this country and Canada, supplemented by a thorough review of American and foreign medical literature made for me by the Nelson Research Bureau.

Before reporting my own case and others collected by me, I will briefly discuss the etiology, pathology, and diagnosis.

Etiology.—R. Fischl (Prague) ascribes the rarity of ulcer in children to the small amount of acid in the gastric secretions of children and to the motility and the rapid emptying of the stomach. Among predisposing factors may be mentioned the following: burns, especially in young children; wasting diseases, especially tuberculosis; local injuries, e. g., swallowing bones, needles, caustics, hot food; local devitalized conditions, e. g., purpura, hemophilia, leukemia, septicemia, scorbutus, anemia, and chlorosis in young girls; follicular gastritis (3 cases reported by Holt).

Pathology.—The pathology of peptic ulcer in children does not differ markedly from that in adults. The ulcers may be simple or multiple. In very young children they are more often superficial erosions; and the deep funnel-shaped ulcer, so characteristic in adults, is rare. In older children more typical ulcers are found, and perforation is common. Ulcers in children are relatively less frequent in the pyloric region. They are found most often on the posterior wall, near the cardia and along the greater curvature. Perforation in the majority of reported cases, confirmed by autopsy or operation, occurs through the posterior wall into the bursa omentalis, through the lesser curvature into the pancreas. Many of the re-

ported cases have been associated with tuberculosis; and it is often difficult to distinguish simple ulcer from tubercular erosions.

Diagnosis.—The possibility of gastric ulcer should be kept in mind in the examination of every child presenting abdominal symptoms. Appendicitis is the condition most often confused with gastric ulcer. Ulcer has been overlooked at operation and discovered at autopsy. Pain in the epigastrium, hematamesis, and bloody stools are conclusive signs. The two latter are usually absent, and pain is often indefinite and not well localized. Pain, worse at night, loss of weight, and chronic indigestion are usually present. These symptoms should suggest ulcer; and physical examination, aided by microscopic examination of the stomach-contents and the stools for concealed blood, will render the diagnosis conclusive. In my case the diagnosis was made through palpation of the pyloric region of the stomach. During a paroxysm of pain the gas could be felt to gurgle through the pyloric opening, and was followed by immediate relief of pain. At varying intervals the child would cry out with pain. Relaxation and complete relief would follow the escape of gas through the pyloric opening. I made the discovery in a purely accidental way, but I believe it is a most valuable diagnostic sign.

Gastric ulcer must be differentiated from pleurisy, empyema, acute gastro-enteritis, and appendicitis. Routine examination of the chest in suspected abdominal lesions will eliminate chest conditions simulating ulcer. Acute gastro-enteritis can be excluded by microscopic examination of the stools for concealed blood. Appendicitis, which has been often confused with ulcer, can be excluded by a differential blood count and examination of the stools. The pain of

appendicitis is often in the epigastric region, and is rarely so well localized as in adults.

REPORT OF A CASE

Lillian B., aged 13, had never been robust and had never menstruated, but up to January, 1912, was in normal health, and engaged in all of the usual activities of children. At this time she was suddenly seized with pain in her left side. Pain was so severe that she cried out. A diagnosis of pleurisy was made at that time. She soon complained of pain in the abdomen which she localized at times in the epigastrium and at other times in the lower abdomen. Appetite was good, and food did not seem to distress her. She vomited but once, and there was no macroscopic blood. Her chief complaint was pain, worse at night and paroxysmal in character. During the paroxysm she would cry out, and place her hand over the upper abdomen. The child was hungry and constipated.

Physical Examination.—She is an anemic, emaciated girl of normal intelligence. Heart and lungs, negative. Scaphoid abdomen. Slight tenderness in epigastrium. Appendix, palpable and slightly tender. No abdominal rigidity. Kidneys, palpable but not tender. Black tarry stools. Two blood counts on successive days showed 18,000 to 9,000 leucocytes. Stomach analysis: free hydrochloric acid slightly in excess; blood present; microscopic examination of stools showed blood.

Surgical Treatment.—On April 18th the abdomen was opened by right rectus incision. Stomach was found adherent for its entire length. Adhesions were especially dense at the pylorus and cardia. They were of cartilaginous hardness at the pylorus; and it was impossible to draw the stomach outside of the abdominal cavity. Posterior gastro-enterostomy was impossible; and an anterior operation was done with a long loop of jejunum. Even this was difficult on account of the immobility of the stomach. An adherent retrocecal appendix, covered by a veil of adhesions, was removed.

Post-operative Results.—All pain ceased after twenty-four hours. Food did not distress; digestion, good; patient very hungry; bowels moved daily after the third day without cathartics. For two months the patient remained well, and gained fourteen pounds in weight. She then began to fail. There was little pain; but all food was vomited, and there was rapid emaciation. Her condition grew steadily worse

until July, 1913, when I re-opened the abdomen, hoping to re-establish drainage, which Röntgenograms showed was faulty. At the second operation, the stomach, omentum, and transverse colon were so densely adherent to the abdominal wall that it was impossible to expose the stomach wall. The retroperitoneal glands were enlarged, and all tissues involved were of cartilaginous hardness. I concluded the condition was malignant and inoperable. There was slight improvement after the operation; and then there was steady decline until two weeks after operation, when she died of starvation.

Post-mortem.—Deep ulceration on greater curvature where the transverse colon, omentum, and jejunum were densely adherent. Infiltration of new growth around the healed ulcer resembled cartilage. Pylorus was surrounded by this same dense tissue. Liver was studded with metastatic growths. There was also a general metastasis, involving the peritoneum, the small intestine, the cecum at the site of the removed appendix, kidneys, retroperitoneal glands, etc. Dr. Stanley P. Black made a diagnosis of primary sarcoma of the stomach from a gross specimen, but on microscopic examination the tumor proved to be carcinoma, secondary probably to round ulcer of the stomach.

CASES REPORTED IN THE LITERATURE AND ADDITIONAL CASES COLLECTED BY THE AUTHOR

There have been gathered from the literature about 60 cases of gastric ulcer in children. These are reviewed by Stowell (*Medical Record*, July, 1905, p. 52), by Cutter (*Boston Medical and Surgical Journal*, October, 1904) and by Jacobi (*New York Medical Journal*, October 30, 1909). I have collected, in addition to these, about 50 cases from the literature, and 15 cases, not previously reported, through correspondence, making about 125 cases in all.

I shall report only those cases treated surgically at this time; and leave the remaining cases to be reported in the Transactions. Dr. F. B. Lund (*Boston Medical and Surgical Journal,* December 23, 1909) reported a case operated on by him. He was able at that time to find only two other cases treated surgi-

cally. I am able to report the following cases subjected to surgical treatment.

OPERATED CASES

1. Author's case described above.

2. Bechtold, C.: Jahrb. f. Kinderh., 60:347 (1904). One case, a five-year-old girl—five months before acute attack, she showed loss of appetite, general emaciation, and paleness, with slight pain in the abdomen. While playing she was seized with severe pain in the abdomen, and symptoms of appendicitis with peritonitis. Operation and death. The autopsy showed perforation of a round ulcer of the stomach.

3. Cheyne, W., and Wilbe, H.: Case of perforated gastric ulcer in a boy. *Lancet,* 1904, 1:1641. One case, a boy, aged 13. Attack began with abdominal pain, becoming more severe. No vomiting; diagnosis of appendicitis. Operation: appendix inflamed, but not perforated; removed. Incision also made over the stomach. Round perforation found on the anterior wall near the cardia. Perforation closed with catgut. Cure.

4. Paterson, G. K.: Ruptured gastric ulcer in a boy, aged 12 years. *Scot. Med. and Surg. Jour.,* 19:228 (1906). One case, a boy, aged 12 years and 3 months. Illness began with pain in the epigastric region; continued for seven days, and then a severe attack of pain in the abdomen with pain in the left shoulder also. Vomiting and symptoms of peritonitis. Operation: perforated gastric ulcer found on the anterior wall of the stomach about two inches from the middle of the lesser curvature; peritoneum washed out; drainage; lung complications, but final recovery.

5. Von Eiselberg (1905. Exact reference not given): Female, aged 12 years. Pain and vomiting of blood for a year. Dilatation of stomach and stenosis of pylorus. Gastroenterostomy done with recovery.

6. Dr. Jas. R. Eastman, Indianapolis (personal correspondence): Child, aged 13. Symptoms—pain, hemorrhage, emaciation. Diagnosis prior to operation: Rodman ulcer operation. Result, benefited.

7. Dr. A. E. Halstead, Chicago (personal correspondence): One case. Patient, aged 11. History of ulcer from first year of life. Severe hemorrhage at 3½, 4, and 5 years of age. Vomiting, nausea, emaciation. From seventh year on evidence of pyloric obstruction, distension of stomach. Enlarge-

ment of abdomen; motor function impaired. Meal retained after twelve hours. Diagnosis made at age of 4, with diagram of probable ulcer after second severe hemorrhage. Treatment: gastro-enterostomy. Large cicatrix, indicating saddle ulcer at pylorus. Recovery.

8. Dr. F. B. Lund (*Boston Medical and Surgical Journal*, pp. 930, 931, Dec. 23, 1909): Boy, aged 8. Bottle-fed. Symptoms: pain in the epigastrium, not related to meals, worse at night; vomiting of blood; tarry stools. Temperature, 101° to 102° F. Diagnosis made prior to operation. Treatment, medicinal; rapid improvement, but symptoms soon recurred. Operation: gastro-enterostomy, posterior short loop; large indurated ulcer, near pylorus on lesser curvature; firm adhesions to liver. Result, recovery.

9. Dr. Van Buren Knott, Sioux City, Iowa (personal correspondence): Two cases, ages 10 months and 5 years. Symptoms, hemorrhage, blood in the stools. Diagnosis made before operation in older child. Treatment, in child of 5, gastro-enterostomy; younger child, medicinal.

10. Dr. Dudley Tait, San Francisco (personal correspondence): One case, 1904. Age, 13. Symptoms, local and general signs of perforation. Diagnosis of peritonitis made before operation. Treatment, high laparotomy, with simple drainage. Recovery without relapse.

11. Dr. Jacob Geiger, St. Joseph Mo. (personal correspondence): Two cases, male, aged 8; female, aged 12. Symptoms, gastric distension, loss of flesh and strength, hematamesis, and bloody stools. Diagnosis made prior to operation. Treatment, gastro-enterostomy. Result, not stated.

I have collected the following cases from the medical literature of all countries through the aid of the Nelson Research Bureau.

Adler, H.: "Gastric Ulcer in Childhood" (*Amer. Jour. Med. Sci.*, 133:135, 1907). One case, a girl, aged 8. For ten weeks repeated attacks of pain in the epigastric region, occurring both before and after meals, nausea and vomiting; blood in vomitus several times; stools also showed blood. Five weeks ulcer cure with rest in bed, careful diet, and bismuth. Recovery, with no recurrence in five months. Marked improvement in general condition.

Barker, C. F.: "Case of Multiple Ulcers of the Stomach in a Child" (*Brooklyn Medical Journal*, 16:519, 1902). One case,

boy, aged 10. A few days previous to acute attack, complained of heaviness in his stomach; relieved by cathartic. Acute illness with prostration came on suddenly; death in a few hours. Autopsy showed four ulcerations in the stomach, two of which had perforated.

Bechtold, C., cites the following earlier cases:

1. Guntz (*Jahrb. f. Kinderheilk.*, vol. 5, 161, 1862): A five-year-old boy, never very healthy, gradually lost flesh, frequently vomiting, with occasional streaks of blood. After an attack of scarlet fever, bloody stools. Death. Autopsy showed adhesion of the posterior wall of the stomach to the colon and a perforating ulcer on the posterior surface of the stomach near the greater curvature.

2. Reimer (*Jahrb. f. Kinderheilk.*, vol. 10, 289, 1876): Girl, aged 3½, with digestive disturbances for over two years. During an attack of measles, diarrhea, nausea, and gastralgia, followed by gastric hemorrhage and death. Autopsy showed stomach adherent to the pancreas, and a round ulcer on the posterior wall of about 4 cm. diameter. Recent peritonitis.

3. Chvostek (*Archiv. f. Kinderheilk.*, 1882, p. 267): Boy, died at 18, but had symptoms of gastric ulcer, attacks of severe pain and constipation since the age of 3, after exposure to severe cold, and was backward in general development. Autopsy showed two ulcers, one at the cardia, the other at the pylorus. Pylorus narrow and musculature hypertrophied.

4. Eröss (*Jahrb. f. Kinderheilk.*, 1883, p. 318): A girl, aged 12, dying of acute miliary tuberculosis; frequent gastric pain complained of. Appetite, small. Section showed round ulcers in the stomach, one perforating into the bursa omentalis, which Eröss considered to be non-tuberculous.

5. Wertheimen (*Jahrb. f. Kinderheilk.*, 19:79, 1882): A girl, aged 10, showed symptoms of ulcer of the stomach (loss of appetite, pain, and bloody vomit, and stools). Recovery in five weeks with careful dietetic treatment.

6. Malinowsky (*Gazeta lekarska*, 1886, No. 6): A girl, aged 8, died. Symptoms of gastric ulcer, especially vomiting of a large quantity of blood. No autopsy reported.

7. Cade (*Rev. Mens. d. Mal. d l'Enf.*, 16:57, 1898): Autopsy of a 2½-months-old boy who had suffered from yellowish diarrhea and vomiting, showed a round ulcer on the posterior wall of the stomach that had perforated into the bursa omentalis, its anterior portion adherent to the liver.

Bisset, A. G.: "Perforated Gastric Ulcer in an Infant" (*Lancet,* 1905, 2:78). One case. Boy, 45 hours' old, six hours before death began to bleed from the bowels, rapidly collapsed, and died. Autopsy showed typical gastric ulcer of acute form, with clean-cut margins, on posterior wall of the stomach, near lesser curvature, about one-half inch from the cardiac opening. Perforated.

Bowes, L. M.: "Gastric Ulcer in Childhood" (*Amer. Jour. Obstet.,* 61:534, 553, 1910). One case, girl, aged 8. Illness commenced by nausea and vomiting and occasional sharp pain. Acute attack with severe pain in epigastric region and vomiting. Vomitus acid and contained blood. Also blood in the stools during the attack. Pain controlled by morphine; alkalies given to neutralize acid. No recurrence in four months.

Cautley, E.: "Case of Gastric Ulcer" (*Society Study of the Diseases of Childhood*—Reprint, 6:225, 1906). One case, girl, aged 5 years and 4 months. Sometimes pain in the abdomen. Attack began with vomiting blood, stool constipated, black. Anemia. Enlargement of spleen. Improvement and cure under treatment. No return of symptoms. Diagnosis not certain, but author believes it to be a case of simple gastric ulcer.

Cackovic, M. von: "Ueber das Ulcus Ventriculi im Kindesalter und seine Folgen" (*Archiv. f. klin. Chir.,* 98:301, 1912). Cackovic examined 172 cases operated on for gastric ulcer in regard to the first appearance of symptoms. He found that symptoms of ulcer appeared in 4 cases before the age of 10, in 9 cases between the ages of 10 and 14, or in 13 cases (7.55%) in all during childhood. He reports but one case before puberty, as follows: A female, aged 13. Pain began at age of 11, vomiting at the age of 12.

Cutler reports 3 cases from the Massachusetts General Hospital, and in addition 15 others, not included in my report but reference to original paper is given above.

Rufz (*Gaz. Med.,* 1843:673. Cited by Rilliet and Barthez. *Revue des Maladies de l'Enfance,* 2d ed., p. 583): Girl, aged 13. Symptoms of dyspepsia. Death sudden from peritonitis. perforation of the stomach in the smaller curvature, following an ulcer almost two inches broad.

Leith (*Internat. Clin.,* 4th ser. 4:49): A girl, aged 10. A month after recovery from croupous pneumonia, began to cough; left-sided empyema developed, causing death. Autopsy

showed empyema cavity on the left side with perforation of the diaphragm, also blood in the intestines, and an ordinary perforating ulcer, nearly circular, in the stomach, just in front of the greater curvature, 4½ inches from the cardiac end. Ulcer adherent by its floor to the spleen, and a suppurating track passed upward and inward between the adherent spleen and stomach directly to the perforation in the diaphragm.

Donné (cited by Rilliet and Barthez. *Handbuch der Kinderkrankheiten*: also by Rhen.): Girl, aged 3. Autopsy showed "in the midst of a punctuated redness within the upper half of the small curvature, a little bit depressed beneath the surface of the mucous membrane, the size of a ten-sou piece. Divergent rays seemed to run out from its center in every direction. The depressed spot was also in folds and the fibrous tissue was thickened, showing a cure from a simple lesion."

Goodhart (*Path. Soc. London Trans.*, 32:79, 1881): Newborn infant, sex not stated. Vomited blood, and died in 30 hours. Autopsy showed stomach distended by blood clot, with small oval ulcer about one-eighth inch long at the cardiac end close to the greater curvature; edge of ulcer very sharp; at the bottom an open artery. Cutler says he includes this case among the chronic ulcers from the "description of the ulcer. Perhaps it should be classed among the acute cases."

Lebert (*Krankheiten des Magens,* 1878), giving statistics for Breslau, includes one case in his table of chronic simple gastric ulcer between the ages of 5 and 10 years, but no detailed statement of the case.

Delore (Ulcère de l'Estomac chez les Nouveau-Né. *Lyon Med.*, 94:301, 380, 1900) describes a post-mortem specimen of case reported in 1866 to the Société de Méd. de Lyon. Infant, a few days old, died of intestinal hemorrhage. Autopsy showed several ulcers of the stomach, invading only the mucosa.

Faulhaber (Zur Diagnose und Behandlung des chronischen Ulcus Pylori. *Munch. med. Wochenschr.*, 60:915, 983. April 29 and May 6, 1913) reports, in all, 32 cases of ulcer of the pylorus, 2 of these in children, as follows:

Case 1, female, aged 4. Periodical pain and digestive disturbances since birth. Diagnosis by Röntgen ray. Medical treatment without cure. Case 2, male, aged 5. Pain and digestive disturbances for a year. Diagnosis by Röntgen ray. Medical treatment.

Fischer, L.: "Two Cases of Gastric Ulcer" (*Archives Pediatrics,* 17:684, 1900):

Case 1, girl, aged 9. Generally backward in development. For some time, bloody vomiting, tarry stools, stomach distended and tender. Prolonged medicinal and dietetic treatment for ulcer. Cure.

Case 2, girl, aged 14. Long ill health. For 3 years vomited blood at times with frequent attacks of pain and vomiting after eating. Improvement under treatment, but relapse when she returned to work.

Francine, A. P.: "Incidence of Gastric Ulcer and Duodenal Ulcer in Philadelphia" (*Amer. Jour. Med. Sci.,* new ser. 129: 429, 1905). Francine in his summary of the cases of gastric and duodenal ulcer in the Philadelphia Hospital that came to autopsy in 1893-1902, gives the following cases in children, forty-two cases in all of gastric and duodenal ulcers:

Case 5, male, aged 11. Stomach shows a few small ulcers; tuberculosis of lungs and intestines.

Case 24, female, aged 10 months. Small round superficial ulcer in stomach; gangrenous stomatitis; diphtheritic gastroenteritis.

Case 25, female, aged 3. Small ulcer in the duodenum. Enterocolitis.

Case 26, female, aged 20 months. Near the cardiac end in relation to the lesser curvature, an ulcer which measures ½ cm. in diameter, cup-shaped with slightly indurated edges. Lobar pneumonia.

Friedenwald, J.: "Clinical Study of a Thousand Cases of Ulcer of the Stomach and Duodenum." (Assoc. Amer. Phys. 27:280, 1912. Also in *Amer. Jour. Med. Sci.* 144:157, 1912.) Friedenwald, in his series of 1,000 cases, had 2 cases of gastric ulcer, both males, under 10 years of age. Average duration of the symptoms, half a year; no hemorrhage. No more detailed description of the cases given.

Hall, J. N., and Hawkins, T. H.: "Congenital Ulcer of the Stomach" (*Denver Med. Times,* 24:646, 1904-5). One case, female infant, 67 hours old. Within two hours after birth, fluids regurgitated from the stomach, very acid. Much vomiting during the first four days, black with bright-red blood several times; stools tarry from the first. Treatment, soda bicarbonate and bismuth, lime water in milk, adrenalin chloride on

occurrence of bleeding. No hemorrhage after sixth day, and recovery apparently complete. The authors consider the symptoms were clearly those of gastric ulcer.

Immerwol, V.: "Ueber das urämische Magengeschwur im Kindesalter" (*Archiv. f. Kinderh.,* 43:321, 1906). One case, girl aged 5. Illness began two weeks before admission to the hospital, with swelling of the abdomen. Then the face and feet began to swell, and this swelling gradually extended over the whole body. Later, fever, diarrhea, headache, and vomiting appeared. The vomitus in the last few days was dark in color, and contained blood. Coma and death. Autopsy showed a dark fluid in the stomach and four ulcers on the posterior wall. The largest ulcer had sharp edges, and had destroyed the mucous membrane only. The other three smaller ulcers were more superficial, with slight hemorrhagic erosions. Kidneys showed lesions of acute nephritis.

Johnson, W. L.: "Case of Chronic Gastric Ulcer in Childhood" (*Med. Council,* 4:345, 1899). One case, female aged 5. Pain in the stomach, especially after eating, for ten months, sometimes followed by vomiting, blood vomited a few times. Tenderness on pressure at pit of stomach. On careful diet, little improvement, followed by an acute attack with considerable blood vomited. Recovery with rest and very little food, diet carefully regulated. Tubercular tendency in patient, but no indication that ulcer is of tubercular origin.

Leary, T. G. S.: "Gastric Ulcer in Child" (*Intercolon. Med. Jour.,* 12:268, 1907). One case, girl aged 7. Digestive troubles with pain a few hours after meals, sometimes vomiting and nervous irritability. Sudden attack of vomiting blood; tarry stools passed. Treatment, rest in bed, rectal feeding for three days, careful diet. Calomel and soda bicarbonate to be given occasionally, also Blaud's pill. Recovery; no recurrence in several months.

Lincoln, J. R.: "Gastric Ulcer in the New-Born" (*Boston Med. and Surg. Jour.,* 137:178, 1897). One case, girl, when a few hours old, was very uneasy, and in first twenty-four hours vomited bright-red blood. The second day, vomited blood several times, and for several weeks cried almost all of the time, and did not take food readily. Satisfactory food found, and recovery after the second month.

Morris, B., and Feldman, M.: "Case of Hematemesis and Melena in an Infant" (*Brit. Med. Jour.,* 1:352, 1905). Male child, 2 days old, vomited blood, attacks of vomiting provoked

by feeding. Stools also contained blood. Treated with soda bicarb., bismuth, and calcium chloride. Cured. The authors believe this to be a case of gastric ulcer causing the hematemesis and melena.

Nisbet, W. O.: "Gastric Ulcer in Children" (*Charlotte Med. Jour.,* 63:89, 1911). The cases as follows:

1. Patient aged 8 years, sex not stated. Impaired digestion for two years, with several attacks of pain, nausea and vomiting. Before presented for treatment, had vomited blood. Treatment, rest and careful diet, bismuth. Cure.

2. Male aged 8. For eighteen months attacks of pain, epigastric tenderness, vomiting. Just previous to beginning treatment, vomited blood. Treatment as above. Cure.

3. Female, aged 7. Pain and acid eructations after meals for about three months; two acute attacks of pain, nausea, and vomiting. In the last attack, considerable blood was vomited. Death followed the administration of a purgative by the attendant, probably due to perforation.

Parkinson, J. P.: "Perforating Gastric Ulcer in a Young Child" (*Society for the Study of Diseased Children, Reprint,* 1:43, 1900-01). One case, male child aged 2 years and 2 months. Had suffered from gastric symptoms, occasional vomiting, and slight fever for ten days; then suddenly vomited blood, and collapsed. Death. Autopsy showed peritonitis, and an ulcer near the center of the posterior wall of the stomach; punched-out appearance; slightly thickened edges, with perforation at the base; another small ulcer nearby.

Parmentier, E., and Lasnier, A.: "Ulcère Simple Chronique de l'Estomac chez l'Enfant" (*Bull. et Mém. Soc. Méd. d. Hôp. d. Paris,* 3d ser. 6:812, 1908). Parmentier and Lasnier report two cases of their own, as follows:

1. Female, aged 2½. At the age of 4 months, periodic attacks of epigastric pain and vomiting began which persisted and became more frequent. A month before entering the hospital, she vomited blood several times; constipation, with black stools. Retardation of growth and development. Treatment, rest in bed, with liquid diet. Improvement, gain in weight, cessation of vomiting. After 9 months, left hospital cured.

Verliac (*Bull. d. l. Soc. Anat.,* 1865:63). Male, aged 14. Digestive troubles for two years, with pain, vomiting, and constipation. Attacks of pain and vomiting became very severe, bloody stools several times, but no blood in the vomitus until just before death. Autopsy showed a perforated ulcer on the

posterior wall near the cardia; and four other ulcerations,—three in the lesser curvature and one in the greater curvature.

Paterson, G. K.: "Ruptured Gastric Ulcer in a Boy Aged 12 Years" (*Scot. Med. and Surg. Jour.,* 19:228, 1906). One case. Boy, aged 12 years and 8 months. Illness began with pain in the epigastric region, continued for seven days, then a severe attack of pain in the abdomen, with pain in the left shoulder also. Vomiting and symptoms of peritonitis. Operation. Perforated gastric ulcer found on the anterior wall of the stomach about two inches from the middle of the lesser curvature. Peritoneum washed out; drainage. Lung complications, but final recovery. Paterson also cites the following case:

Barthaz and Sanné (*Maladies des Enfants,* 3d ed., 2:367). Female, aged 13. Dyspeptic symptoms and vomiting. Death from perforation of an ulcer on the lesser curvature of the stomach.

Reichelt, J.: "Ulcus Ventriculi im Kindesalter" (*Wien. Med. Presse,* 46:126, 1905). One case, boy aged 8. For some years, vomiting and constipation. For a year, a burning pain in the stomach, and vomiting after eating. Emaciation. Death after repeated severe attacks of pain and vomiting. Autopsy showed an ulcer with sharp edges in the stomach 1 cm. above the pylorus in the lesser curvature, perforated; a second ulcer on the posterior wall at the pylorus, base formed by adherent head of the pancreas.

Robinson, J. A.: "Ulcer of the Stomach in a Boy, Aged 14" (*International Clinics,* 15th ser., 3:119, 1905). One case, male, aged 14. Ill for about one year. Pain after eating; vomiting of mucus and blood, vomiting becoming more and more frequent, and the patient becoming anemic and emaciated. Rest and dietetic treatment. Temporary improvement, but after eating solid food on one occasion, vomited bright-red blood. After that, progressive failure in spite of careful treatment and rectal feeding. Death. Autopsy showed ulcer at the pylorus. The lining of the stomach was fibrous tissue, the stomach was contracted and the pylorus was constricted.

Von Eiselberg (1905, exact reference not given): Female, aged 12. Pain and vomiting of blood for a year. Dilatation of stomach and stenosis of pylorus. Gastro-enterostomy done, with recovery.

DISCUSSION ON THE TWO PRECEDING PAPERS

Dr. H. M. Richter (Chicago, Ill.): It seemed to me, while Dr. Hill was reporting his cases, that his mortality really is materially less than what he believes it is, since the complications of meningitis and jaundice really mean something more than simple pyloric obstruction. In my own work I was fortunate enough to have uncomplicated cases. I saw my cases, some early, some late, but they were all straight cases of pyloric obstruction.

There are four points I should like to mention briefly, in connection with pyloric stenosis as bearing upon Dr. Hill's paper. It has been mentioned to me a number of times that we are likely to find the things we are looking for. Of the 22 cases of pyloric obstruction I have reported, 19 were of the true hypertrophic type. Of the 19 cases, in all but one, we were able to palpate a tumor before opening the abdomen. This does not leave any room for imagination.

A second point is with reference to medical treatment of these cases. It is probable that by prolonged medical or dietetic, or some other treatment a number of these babies could be saved from being operated upon; but the mortality of the total number of cases would be materially higher. The reported statistics give a mortality from the various operations for cases of this sort as somewhere around 50 per cent. The mortality of the unreported cases is something like 80 or 90 per cent. Of my own knowledge I know of 8 consecutive cases of pyloric stenosis in infancy, operated upon by seven men, with seven deaths. Preliminary medical treatment increases the mortality by reducing the condition of those children that finally come to operation.

A third point is the final condition of these children. Of my 22 cases, 19 survived the operation, and 3 died from the operation itself. Of the 19 that survived the operation 1 has since died; 18 are alive, and of the 18 I am in touch with every case. Of the 18, 16 were operated upon six months to five years ago, and of these, 15 are above the average weight of children of their age, as given by the various tables of pediatricians. Two cases too recent to report upon as to end-results were operated on within the last two months, and have been gaining from one to two ounces per day.

A fourth point is this: Last month during the Congress of Clinical Surgeons of North America, held in Chicago, Dr. Case skiagraphed ten of my babies, ranging from eight days to three and a half years after operation. Of this number,

9 were gastro-enterostomies. In every one of the nine gastro-enterostomies the bismuth passed through the gastro-enterostomy opening, not through the pylorus, which seems to suggest permanency of the closure of the pylorus.

Another point: I have one case of spasm skiagraphed subsequent to operation. The spasm case was operated on because of rapid loss of weight, although there was no tumor. It was skiagraphed the eighth day after gastro-enterostomy was done, and bismuth passed not by way of the pylorus, but through the gastro-enterostomy opening.

Dr. Grant: How many gastro-enterostomies have you done?

Dr. Richter: I have done 19 gastro-enterostomies. Three operations were done on the pylorus, 2 being submucous pyloroplasties. I believe gastro-enterostomy is the preferable operation. Of course, an accident may happen, but it is not likely. In my 19 gastro-enterostomies I had 2 deaths.

Dr. Daniel N. Eisendrath (Chicago, Ill.): The subject of congenital pyloric stenosis is a very live one, because no single operator has had very much experience regarding it. I have had three cases. I think we owe a great deal to Dr. Hill and to Dr. Richter for reporting such a large number of cases. Dr. Richter's experience has been larger than that of any other man living in this regard.

The point made by Dr. Richter of bismuth going through the gastro-enterostomy opening, in preference to the old pyloric opening, is interesting, and shows why the pediatricians should turn over these cases to us at an early date, namely, because the anatomical conditions are such that they cannot hope to recover without operative treatment. Anyone who has dissected one of these tumors at autopsy knows that the tumor that is present at the pylorus is one which is so large that you can often feel it through the abdominal wall. It forms quite a nodule, so when the baby is relaxed you can feel it as a limiting point from which the peristaltic waves start. A cross-section will show the lumen narrowed down to almost nothing; and then there is hyperplasia of the muscular coat, the principal involvement being the muscular and submucous coats. These are not medical cases. The pediatrician should co-operate with us in these cases. Here is a difficulty referred to by Dr. Richter. Pediatricians, a great many of them good men, claim that a large number of these cases are pylorospasms, that by proper feeding and proper treatment the

spasm of the pylorus can be overcome and the children will get well without operation, but, unfortunately, many persist in this view until it is too late to do any good surgically. Personally, all of my cases have reached me in such a condition of inanition that I have been able to save only one out of three cases. We should report our bad results as well as our good ones.

There is one point I would like to emphasize, and which has not been brought out, namely, these are not gastro-enterostomy cases upon which we should use clamps. I am confident that Dr. Richter and Dr. Hill will indorse my statement when I say that the duodenum or jejunum in these cases is not much larger than the adult's finger. The stomach itself is much thicker and much larger, but I have succeeded in these cases just as well without the use of clamps as with them, and I know of two cases that have died of perforation following the use of the small sized ordinary gastro-enterostomy clamp, and I would like to emphasize this point, because some of you may have had experiences of this kind. Dr. Richter uses the clamp only on the stomach, and not on the duodenum. Death was not due to any local condition, but to septic aspiration—pneumonia.

In regard to the method of operation, it seemed to me from the autopsy specimens that the method suggested by Rammstedt and Weber, of dividing all of the coat, down to the mucous membrane, so that everything springs apart, was an excellent method until I talked with Dr. Richter, and he told me that in one of his cases he had perforation and death in which he carried out this idea. I agree with him that gastro-enterostomy is the preferable method, but it must be done quickly and we must use small needles and smaller forceps than we usually do.

Dr. Charles H. Mayo (Rochester, Minn.): This report of cases of pyloric stenosis is most interesting. It seems to me, however, that the most important issue which arises is a side one, such as that which Dr. Lockwood makes in his reported case of carcinoma in a little child. I think such a case far outweighs the question of gall-stones.

I would say to Dr. Lockwood that I believe his is one of the most important communications which has been presented for some time, namely, a case of carcinoma reported in a child of seven after examinations by a pathologist. I should suggest to Dr. Lockwood that when he returns to California he send sections of these tumors to Adami, LeCount, and Welch. The

diagnosis should be confirmed absolutely by men of the highest and best reputation, or it should be disproved.

Dr. Willard Bartlett (St. Louis, Mo.): Regarding one case of pyloric stenosis: It is interesting on account of the age of the child. This boy had been able to get along and reach the age of twelve years, when I did a gastro-enterostomy. He had a typical mummified appearance such as I had never seen before, and was a boy about one-half the size he should be for his age. While he lived and I kept track of him for some months after the operation, the result was not as good as we are accustomed to see in these cases. For some reason he did not develop as those babies do who are operated on earlier in life.

Dr. W. D. Haines (Cincinnati, Ohio): After analytical study of the cases referred to by Dr. Hill in which the mortality following operation was said to be 51 per cent, I am thoroughly convinced that it was due to lateness of the operation. In other words, the child was ready for the undertaker before it was turned over to the surgeon. I reported a case at that time which was in the nature of a post-operative accident. It was a borderline case. In looking up these cases I have found in 7 out of 152 that were reported the wound failed to heal for many days after operation; and, at some time during the post-operative recovery a loop of intestine came out through the abdominal-wall incision. This occurred in our case on the night of the fifth day after the operation. A loop of the intestine came out, we removed a suture, and the loop of the intestine was replaced. There had been no attempt at union. I think the child's general health was so low, and the reparative processes were of such little value, that this portion of the wound failed to heal promptly. The child recovered, and is today strong and healthful.

With reference to the technic: It is hardly necessary to say that this operation should be done without clamps, as has been spoken of by one of the preceding speakers. In this type of case in a child that is weak, I think the less we do, and the quicker we do it, the better.

Dr. Bunts is still doing, and has always done, the anterior (long loop) operation both in adults and in children; and he told me at the time I reported my case to the State Medical Association, that all of his cases had been anterior long-loop gastro-jejunostomies. Pyloroplasty or dilatation was impos-

sible in our case as the pylorus and the first portion of the duodenum were not of greater diameter than a lead-pencil.

From the report of successful cases, I believe we shall get these cases earlier in the future, and I think it will only be a short time until our mortality will be reduced very materially in this type of operative work.

Dr. E. P. Quain (Bismarck, N. D.): I wish to report a case of pyloric stenosis in an infant, on which I operated within the past year. The case presents two points of interest: first, the unusually early emesis after birth, and, second, the method of treating the stenosis.

This baby boy vomited immediately after birth, and the vomiting was repeated several times before he had been given anything whatsoever to swallow. This fact of early emesis, it seems to me, points directly to a prenatal or *congenital* condition in this case. The material vomited was reported by the nurse to be a "dark liquid." The delivery had been normal and easy.

After the initial emesis before the first nursing, there was no emesis until the fourth day, when the baby began to vomit after nursing. This increased in frequency until the beginning of the third week, when practically all food was expelled. Careful medical treatment, including lavage and artificial foods, gave no improvement.

I saw the child first on the seventeenth day after birth. He looked miserable and emaciated, having been reduced in weight from seven and a half pounds at birth to less than six. Obstructed gastric peristalsis was evident, the stomach rolling up like a hard ball toward the right side of the epigastrium every few minutes.

In spite of the doubtful surgical risk, operation was undertaken on the nineteenth day. The abdomen was opened under local anesthesia, a little ether being given during the work on the pylorus. A hard ring displacing the normal pylorus was readily seen to be the cause of the obstruction. The stomach wall was greatly hypertrophied. I made a longitudinal incision over the pylorus through the muscularis, and dissected it carefully and bluntly away from the mucosa. By compressing the duodenum on one side and the stomach on the other, I was then able to force air from the stomach through the opening, and by using a little force it was seen that the air-pressure within caused the mucosa to bulge out between the edges of the cut muscularis.

It was found difficult to close the incision transversely, owing to the thick and unyielding muscularis; but, after a diamond-shaped area, taking in the greater part of the anterior surface of the pylorus, had been excised, the transverse closure was easily made with interrupted linen suture. Air was again forced back and forth through the pylorus to prove the amplitude of opening. Aside from a few vomiting spells on the day of operation, and a delayed healing of the abdominal wound, the baby made a fairly uneventful recovery. It gained rapidly in weight and well-being, and, at the age of eight months, scored over 90 per cent at a recent baby contest in our city.

The arguments I should like to present are, that a gastro-enterostomy is a grave operation for a little emaciated infant, and that, if it is possible to overcome the obstruction in a reasonable percentage of cases by this simpler method, which does not open any mucous membrane, submucous pyloroplasty should be the procedure of choice.

Dr. Clarence M. Nicholson (St. Louis, Mo.): If it be a fact that congenital and spasmodic stenosis of the pylorus constitute about 8 per cent of all cases, and that the hypertrophic form gives rise to the other 92 per cent, since in the hypertrophic and congenital forms absolutely nothing is to be gained by medical treatment, it would seem that medical treatment should cease when the diagnosis is made. While it may be easy to make a diagnosis of hypertrophic stenosis of the pylorus, where a tumor is to be felt, and all or most of the food is vomited, in certain other cases where a considerable amount is passed through the pylorus the diagnosis is rendered more difficult. In that small percentage of cases included in the spasmodic group, medical treatment may be resorted to. I believe divulsion of the pyloric opening is usually attended with comparatively poor results.

I heartily concur in what Dr. Hill has suggested, namely, the operation of gastro-enterostomy for these cases at the earliest possible time. We know that the nervous system in very young children is not well developed, and those who operate on cleft palate shortly after birth know that infants stand operation well.

Dr. Roland Hill (closing the discussion on his part): The question of the operation to be performed in these cases to my mind resolves itself into the operation of posterior gastro-enterostomy. In nine of my cases I did posterior gastro-

enterostomy, and in one I did divulsion. I did the latter simply because the child apparently was dying. In this case one of our pediatric nurses was with the baby the night before and could scarcely tell whether it was alive or dead, and the physician who had seen the case said that if we attempted gastro-enterostomy the child would die. I made a nick in the stomach, got a pair of forceps in the pyloric opening, and divulsed the pylorus. I ran a tube down and poured into it four ounces of sterile water. That child came off the table in better shape than it went on. The stenosis rapidly recurred, and the child died in a week's time.

In view of the last case I had, I believe this child would have had a good chance if I had done posterior gastro-enterostomy. I have worked probably on a dozen dead babies, and I am convinced the operation of posterior gastro-enterostomy is the logical one, and the one that will give the greatest satisfaction in all cases. Operations around the pylorus are not best adapted for this work.

There is one other point that it is necessary to remember in connection with these cases, and that is this: These babies have a low vitality and the gastro-enterostomy opening will heal much more readily than in the abdominal wall. There is a tendency for the abdominal wound to break right open, as occurred in several of my cases. I believe in making the abdominal closure stay; sutures of some kind should be employed. For this work I have used silk-worm gut. That is very important.

In regard to the use of clamps, they are hardly needed. I have used them in my cases, but a clamp on the jejunum, which is so pliable and delicate and empty at the time, is something I probably shall avoid in the future.

The mortality in these cases, when referred to us early, I believe will come down, so that it will be comparatively low.

Dr. Lockwood (closing the discussion on his part): I rather anticipated some questions in regard to this case of carcinoma in a child of thirteen, instead of seven, as Dr. Mayo stated. I brought with me the slides taken from the site of the ulcer and retroperitoneal glands, and also specimens which show mestastases in the liver and retroperitoneal glands.

I took precaution before leaving Pasadena to call up Dr. Black and ask him if he was satisfied that the diagnosis of carcinoma from the slides was correct, and he said he had no question about it. However, I shall follow Dr. Mayo's advice, and will send sections to Dr. LeCount and Dr. Adami.

I considered the possibility of syphilis, although no Wassermann was made. I reviewed the family history carefully. There were six children in this family born since this child and there was no evidence of syphilis in any of them.

RETROCECAL APPENDICITIS

David Walker Basham, M. D.

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I almost feel as if I should apologize for presenting the subject of appendicitis, in any phase whatsoever, at a meeting of this Association, notwithstanding, when we come to think about it, there has been but little written on that form of the disease under consideration in this paper.

Retrocecal appendicitis has reference to the cases in which the vermiform process occupies a position posterior to the head of the cecum. The situation of the appendix vermiformis in these unusual, if not abnormal, positions is extremely variable. In fact it would be difficult to find two cases combining like features in every particular; consequently it would be difficult to classify the various forms of retrocecal appendicitis in such a manner as to facilitate our study and understanding of the malady.

Any attempt at classification of irregular conditions of disease, unless there are sufficient points in common with the different varieties, leads to confusion rather than elucidation of the subject. There are, notwithstanding, as many as three different varieties of retrocecal appendicitis, met with often enough, to justify such a classification; but, even then, it is doubtful if our understanding of the condition is in any way made easier thereby. The most rational classification has reference to the anatomical situation of the appendix. There are three principal locations

where the retrocecal appendix is oftenest found. As the surrounding viscera and other anatomic structures govern, to a very considerable extent, the natural course of the disease, it follows then that the subject is best considered from the standpoint of the anatomic location of the appendix. For example, if the vermiform appendix in its ordinary location becomes infected and goes on to abscess formation, the omentum, the posterior peritoneum, the head of the cecum, and juxtaposed coils of the ileum approach each other round about the diseased organ, become agglutinated, and determine the limitations of the abscess. If, through lack of resistance, the infected organ is not walled in, as just stated, the cavum peritonei rapidly fills with a milky fluid, which, if not evacuated, very soon becomes purulent, results in general peritonitis, and the death of the patient. In a certain proportion of these cases the appendix lies directly posterior to the cecum, but inside of the peritoneal cavity. In another proportion the appendix is situated posterior to the cecum, and at the same time behind the peritoneum. In another class of cases the appendix lies posterior and external to the cecum, but within the peritoneum. The first class comprises perhaps the smaller number of cases. In this class the appendix is almost always incarcerated in a mass of adhesions; and suppuration rapidly supervenes once the appendix is infected. The toxicity of this form is very marked because the appendix lies imbedded in an area richly endowed with lymphatics. In my own experience appendices in this situation have always presented great morphological irregularities as compared with the appendix hanging free from the head of the cecum.

There are certain factors in the diagnosis of this form of appendicitis which, for the sake of clearness, should be mentioned here. The overlying cecum, distended with gas, may easily lead to confusion, because

there is resonance on percussion instead of dullness. There may be little or no rigidity of the abdominal muscles, because the posterior peritoneum, instead of the anterior, is involved. The pain is often reflected outward over the upper part of the thigh, because the nerves supplying that region are pressed upon by the abscess, for, at least in Kansas, these cases usually go on to suppuration before we see them. This variety of appendicitis may simulate ureteral stone, even to the presence of frequent urination and blood in the urine, because the ureter is pressed upon. Pain is often felt down the thigh, and the patient occupies the dorsal decubitus with the right leg flexed to relax the psoas muscle, which lies beneath the inflamed appendix. This form of appendicitis is difficult to recognize, because there is often no dull area, either internal to the anterior-superior spinous process of the ilium or over the head of the cecum.

In the second class mentioned, where the appendix is outside the peritoneum, the organ may lie directly behind the cecum or behind and to the outer side. Several times I have observed the appendix situated directly behind the ileocecal valve. The appendix in these abnormal situations is almost always obscured beneath adhesions; moreover, the abnormally situated appendices are extremely prone to disease.

Appendicitis in the retrocecal extraperitoneal location of the appendix is likely to be mistaken for stone in the kidney, renal abscess from any cause, or some pathologic condition of the gall-bladder. The common form of retrocecal appendicitis is the variety where the appendix is situated posterior and external to the head of the cecum and within the peritoneum. The appendix may be situated anywhere in the space between the under surface of the liver and the iliac fossa. There is usually a good deal of pericolic membrane in connection with these abnormal forms of

appendicitis. This variety of appendicitis is indeed much more frequently met with than is commonly supposed. As a rule, the appendices in this location are very long. These appendices are usually adherent, whether they are inflamed or not. They are for this reason dragged upon by the cecum, which probably accounts for the abnormal length of the organ in this location. I have thought that the appendix in this location, between the os innominatum and the colon, is subject to traumatism from pressure, especially when the colon is distended.

The diagnosis of appendicitis in these abnormal locations is beset with some difficulty. There is generally no pain over the McBurney point excepting that which arises from any intraperitoneal inflammatory process. There is generally no marked rigidity of the right rectus. Acceleration of the pulse rate, elevation of the temperature, and vomiting are present as often as in ordinary appendicitis. The presence of an abscess does not always make the diagnosis easy, for the mass is situated on the posterior wall of the peritoneum or otherwise to the outer side of the cecum, instead of just beneath the anterior wall of the abdomen. There is, therefore, resonance on percussion over the diseased area, instead of dullness. Instead of muscular rigidity limited to the right rectus, there is usually a general sensitiveness of the whole right side of the abdomen. If there is an abscess of the appendix situated external to the appendix, it is located just internal to the anterior-superior spinous process, and extends backward and upward into the iliocostal space. Of course a painful inflammatory condition, whether accompanied by tumefaction or not, in this location should make one think of the possibility of a renal affection or a gall-bladder trouble, as well as of appendicitis. The leucocyte count cannot always help us here, for both renal abscess and empyema of the gall-bladder give

rise to leucocytosis. Post-cecal appendicitis, with the appendicular mass situated directly behind the cecum, may be mistaken for a neoplasm in the head of the cecum or for obstruction of the bowels from some other cause. Here the leucocyte count may be accepted for its full value as a differential point.

Appendicitis in an appendix located behind the head of the cecum, and, perforce, also, just behind the terminal portion of the ileum and Bauhim's valve, causes obstruction of the bowels, which is relieved by timely operation. This form of appendicitis often causes the pain to be reflected outward over the hip and upper part of the thigh along the course and distribution of the lumbar nerves, pressed upon as they cross the psoas muscle. It may be observed that the superficial veins of the right side are unusually full, and more prominent than on the other side. As first pointed out by the lamented Dieulefoy, there is usually albuminuria in this sort of appendicitis.

In the chronic non-suppurative form of post-cecal appendicitis, it may be very difficult to know whether we are about to deal with a gall-bladder lesion or a diseased appendix. I think, in this situation, it is well to place the incision so that both the appendix and the gall-bladder are accessible. The verisimilitude of renal abscess and abscess of a post-cecal appendix is not imaginary; and this sometimes places the operator in an unpleasant situation, especially if some of the patient's relatives insist upon being present at the operation. Here we cannot avail ourselves of the accommodating incision, as in dealing with the gall-bladder and the appendix at the same time. This might be feasible in the absence of suppuration or extensive adhesions about the appendix.

In résumé of the subject: It is well to mention the conditions which may be mistaken for post-cecal appendicitis. It must be remembered that, at least in

Kansas, we usually see these cases after suppuration has taken place. We must therefore bear in mind the possibility of stone in the kidney, abscess of the kidney from any cause, stone in the ureter, tumor of the lower pole of the kidney, affection of the gall-bladder, and obstruction of the bowels, especially in the presence of a neoplasm of the cecum or ascending colon.

In the operative therapy of these irregular forms of appendicitis it is well to place the incision over the most prominent part of the tumor, if there is a mass. If there is no tumefaction the incision should be made so as to give access to the ileocecal region and ascending colon. This is no place for the gridiron, nor indeed for any of the transverse incisions. The drainage, when necessary, should be placed in a stab-wound in the iliolumbar space. As before remarked, drainage is the rule in southern Kansas, for we rarely see the case until there is an abscess.

Posterior drainage in these cases is far superior to the anterior drainage with the Fowler position. The appendix should always be removed in these cases, if at all feasible without compromising the safety of the patient. If the appendix is not removed, drainage is usually prolonged for many weeks, and even then the appendix may have to be removed, in the presence of pus. The pressure and disturbance about the cecum frequently lead to gangrenous perforation followed by fecal fistula. If the drainage is unobstructed these fistulæ generally heal spontaneously.

THE REMOVAL OF THE APPENDIX IN ALL CASES OF APPENDICITIS WITH LOCALIZED ABSCESS

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As many inquiries have been received since the publication of a former article[1] dealing with the removal of the appendix in all cases of appendicitis with localized abscess, I desire again to present the subject for consideration and criticism.

In discussing the subject with many surgeons of large experience I find that the majority still seem inclined to follow the plan which was apparently in universal use many years ago,—that is, simple incision and drainage of the abscess, making no effort to remove the appendix unless it presents itself within the confines of the abscess, practically free from adhesions, and in fact inviting removal. Furthermore, the trend of the majority of articles dealing with this question, as well as the method of handling these cases at the various large surgical clinics, would indicate that by far the greater number of operators remove the appendix only in a comparatively small percentage of such cases.

The reason usually given by operators who follow this course is, that it is unsafe to separate the adhesions forming the boundary of the abscess, thereby, during the search for the appendix, liberating pus and septic material into the uncontaminated areas of the

peritoneum. This was the opinion held by myself up to some six or seven years ago, my former custom being to remove the appendix only in instances in which such removal was easy, and could be accomplished with the separation of few adhesions.

During the past seven years I have operated on 501 cases of appendicular abscess, sharply localized. This series does not include any case in which, at operation, the general peritoneal cavity was found to be infected, or cases of gangrene of the appendix without the formation of a distinct peri-appendicular abscess. In this series of cases the appendix was removed in every instance at the primary operation, whether easily detected within the abscess cavity or deeply buried by adhesions.

In the 501 cases six deaths occurred. One patient, a little girl aged 4, died thirty-six hours after operation, from what cause I do not know, as this operation was done in the country, and I could get no clear post-operative history. A man, aged 49, died sixteen days after operation from cardiac thrombosis. A boy, aged 17, died four days following operation from septicemia. Another patient, a woman, aged 31, died from septicemia six days after operation. A man aged 27, died twelve days following operation from post-operative obstruction, further intervention being refused. The sixth patient, a man aged 24, died from secondary hemorrhage the third day after operation. The ages of the patients included in this series ranged from 10 months to 84 years.

The results secured are infinitely better than those I obtained under the old plan of treatment, in which, in a large proportion of cases, the appendix was allowed to remain undisturbed, with a resultant mortality of 8 per cent, whereas, in the series of 500 cases of appendicitis with abscess, mentioned above, the appen-

dix in every instance being removed, the mortality was 1.2 per cent.

It is to be noted that in the six fatal cases of this series, not a death occurred from peritonitis; yet it seems to be the fear of inviting, or even of occasioning a spreading peritonitis which restrains many operators from removing the appendix in abscess cases. Since the adoption of the Fowler position, and drainage of the peritoneal cavity by means of a large split rubber tube inserted to the bottom of the rectovesical pouch or cul-de-sac, supplemented by saline proctoclysis, peritonitis has been shorn of its terrors.

It was noticed that, in those cases of diffuse septic peritonitis due to perforation of the appendix which were so treated, convalescence was almost uniformly rapid and smooth and without distressing sequelæ. The fear of diffuse septic peritonitis having been removed, the way toward the removal of the appendix was cleared; and the systematic removal of the appendix in all cases became the logical routine procedure.

During the early work in this series it was seen that, during the search for and the removal of the appendix, small pus collections were frequently encountered here and there among the adherent coils of intestine and omentum, whose presence had not been suspected, and which would have been overlooked had not the adhesions been separated. Drainage of the larger pus collection would not have reached these smaller hidden septic foci. These have been found on both sides of the abscess cavity; and as many as eleven distinct pus collections have been noted.

In many instances in which, because of a high location of the appendix, the abscess cavity was high and tightly walled off, free separation of the adhesions forming its floor would reveal a large amount of pus or purulent serum in the rectovesical pouch, at times quite remote from the appendix, which would certainly

have been otherwise overlooked. Thus the reasons have been exposed for many a secondary operation for what has been improperly termed a secondary abscess. Increased experience with the method has tended to confirm in every particular the accuracy of this early observation.

In the series of cases which form the basis for this article, there was but one death from post-operative obstruction. The free separation of adhesions tends greatly to lessen the frequency of this serious and often fatal complication, as it establishes free communication between the region of the cecum and appendix, formerly occupied by the abscess cavity, and the lowermost pelvic peritoneal pouch, permitting the best drainage of both regions by a single tube, and thus doing away with the necessity for multiple drains either of tube or gauze.

The percentage of fecal fistulas has in this series of cases been unusually small. I am not able to give the exact figures, as many of the patients were operated on at their homes; but in those patients operated on in the hospital,—and these were by far the more numerous, the occurrence of fecal fistulas was seldom noted.

This may be at least partially explained as follows: The removal of the sloughing appendix at the time of the primary operation, with proper management of the stump or in some cases the diseased cecal wall itself, has limited the tendency of the gangrenous process to invade adjacent intestinal tissue. Again, the separation of all adhesions, entirely obliterating the abscess cavity and throwing together the space formerly occupied by the abscess and the lower peritoneal pouch, and the insertion of one large tube in the rectovesical cul-de-sac for drainage, renders unnecessary the placing of multiple drains, either of tube or gauze, in the cecal region, where, by exercising pres-

sure on tissues whose vitality is impaired, they may determine the formation of fistulas by pressure necrosis.

With the patient in the exaggerated Fowler's position, all adhesions having been separated, we secure at once the collection of septic fluids in the most dependent portion of the peritoneal sac, whence they are rapidly conveyed to the surface through the large split rubber tube with the loose-fitting gauze wick.

Because of the rapid free drainage so secured, the tendency to post-operative sepsis manifested by hepatic abscess, pylephlebitis, subphrenic abscess, empyema, etc., is much less marked than in those cases in which a leaking appendix is permitted to remain, hourly adding its burden of septic and infectious material to the load which is in many instances already straining the powers of resistance and recuperation to the very limit.

In support of this statement I may mention that Dr. George Ross[2], of Philadelphia, reported twenty-four cases of subphrenic abscess, practically every case occurring in patients in whom, at the primary operation, the appendix had been left, and stated that for four years he had not left an appendix in operating in these abscess cases and that he had had no occasion for regret.

In my experience, a second operation in appendicitis with abscess formation in which the first operation consisted merely of incision and drainage, has been necessary much more frequently than is usually stated in text-books. As nearly as I can estimate, it occurs in about 25 per cent of cases.

I recall one patient, the brother of a physician, who had been operated on four times in three years for appendicitis with abscess, merely by incision and drainage. I removed this man's appendix from a mass of dense adhesions with abscess during his fifth attack,

which occurred also within the three-year limit. At the request of the physician who had performed the initial operation, I have several times operated on patients who had, following incision and drainage, a recurrence of the peri-appendicular abscess before they left the hospital. Others have been operated on for appendicitis with abscess formation, and had the appendix removed months or years following the first operation.

The removal of the appendix at a secondary operation is frequently very difficult because of the density of the surrounding adhesions. In the majority of instances it will be found much easier to remove it at the first operation; and this should now be done rather than subject the patient to a prolonged and often serious second operation with the resulting time loss and unnecessary risk thereby enforced.

Dr. Archibald MacLaren[3], of St. Paul, in a paper on rectal drainage for the relief of certain advanced septic conditions in the lower pelvis complicating suppurative appendicitis, states that, since employing the method above advocated, he has noted the indications for rectal drainage in his cases to be reduced just 50 per cent.

Dr. J. Rilus Eastman[4], of Indianapolis, reports that his results have been much better since he has been removing the appendix in every case of this character; and he most heartily commends the method.

In brief, the technic of the procedure recommended is the following: The incision, regardless of the location of the abscess, is made through the right rectus muscle; and the peritoneal cavity is freely opened. A small gauze pack is tucked into the upper angle of the wound, and so adjusted as to prevent any extravasation of pus toward the median line. A gauze pack is adjusted also from the upper angle of the wound to the lower pelvis to prevent any extravasation toward

the left. The abscess cavity is then freely opened with the fingers, unless opened by the peritoneal incision, and the pus is rapidly mopped up with gauze sponges. The appendix is then exposed and removed. All adhesions separating the abscess cavity from the lower pelvis are now freely separated, and the pus or seropus, which is usually found in the lower peritoneal pouch, is rapidly removed by sponges. A large split rubber tube, carrying a loose-fitting wick of iodoform gauze, is inserted through the lower angle of the wound to the bottom of the rectovesical pouch, care being taken that it does not, at the lower end, impinge on an intestinal coil. The protecting gauze is removed and the wound closed down to the tube with interrupted through-and-through sutures of silkworm-gut. In these cases a continuous-layer suture is greatly inferior to a through-and-through suture because of the danger of infection of the wound. As drainage of the peritoneal cavity can be maintained only for a relatively short time, this drainage must, in cases of this character, be very free; and the use of a large tube, placed as above described, secures this result.

The patient is then placed in bed, the head of which is elevated at least thirty inches from the floor. The patient is turned on his right side inclining toward the prone position; and a special nurse, instructed to see that the right-side posture is maintained for at least twenty-four hours, should be placed in charge. Proctoclysis is immediately begun, and continued as long as needed in each case. Water is allowed at once, but in small quantities. Morphin is given whenever necessary to control pain. No nourishment is given for at least forty-eight hours, and then it is given sparingly.

The cases which furnish the material for the conclusions reached in this article represent an unbroken series of similar treatments, as I have removed the appendix in every case of appendicitis with localized

abscess since the first one of the series was removed.

Many of these patients were operated on in their homes, frequently under most unfavorable conditions; and I feel that the series furnishes a severe test of the method.

In considering the mortality of this procedure, which, as previously remarked, has been, in this series, 1.2 per cent, we not only must compare with the primary mortality of similar cases treated by drainage alone, but also must add to this latter mortality that attending those cases in which, because of recurrence of disease, a second or a third operation becomes necessary. In my experience, not only has the mortality been greatly reduced by this plan of treatment, but also convalescence has been much shortened.

The procedure is not advocated for the operator with little experience in abdominal surgery; but, it may be added, no man of little experience should attempt to operate on any acute suppurative process within the abdomen. The technic was, I freely admit, adopted as a routine measure with some misgiving at first, and I rather expected to be forced to discontinue it, at least in certain instances, but as experience with it increased and it was noted that adhesions in these cases could be freely separated, not only with impunity, but also, under proper precautions, even with benefit, my faith in it has been strengthened, and I no longer hesitate to offer it as a routine procedure to the competent surgeon, for I firmly believe that, by its employment, convalescence will be shortened, post-operative sequelæ will occur less frequently, and mortality be reduced.

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DISCUSSION OF THE TWO PRECEDING PAPERS

Dr. Joseph Rilus Eastman (Indianapolis, Ind.): I think no one need apologize for presenting the subject of appendicitis, because there is still room for progress along the line of treatment of this disease. I realize that I am not the proper person to discuss the subject, because there are many men present who have had a greater experience than mine, both as to fibre and volume. But there is a reason why I was designated by Dr. Knott to open the discussion, and that reason is this: I had written a short article reporting the gratifying improvement which I had made in my statistics after the adoption of Dr. Knott's plan. Prior to the reading of Dr. Knott's paper we had been surfeited with papers and discussions dealing with the question of when to operate in appendicitis. We had it in the forum of the American Medical Association from diametrically opposite positions, some taking the position that all cases should be operated on on the spot, and others contending that the patient should be nursed along, starved, and so on according to the expectant plan. Dr. Knott came forward with a definite plan of dealing with a special phase of appendicitis. Instead of attempting to deal with the whole question, he brought before us the subject of appendicitis with localized abscess, and presented, I think, a most satisfactory plan of dealing with it. I do not subscribe to the proposition that the appendix should be removed on the spot in all cases of appendicitis, but I do subscribe to the statement of Dr. Knott that when we operate for appendicitis we should get the appendix. I have gone a little further than Dr. Knott, and in some cases of generalized suppurative peritonitis have devoted as much as an hour to separating intestinal adhesions and wiping out little puddles of pus and finding the appendix finally; and I have been gratified to note a great improvement in my personal statistics.

I think there is a difference in the virulency of the bacteria that are in the pus and those that are in the appendix itself. I know of no research work which has proven that there are antibodies or lysins, or what not, in the pus itself which reduce the virulency of these bacteria, but clinically I believe there is evidence enough to justify me in saying that the bac-

teria that are in the appendix itself are decidedly the more virulent, so that I do not fear pus very much, but I do fear the appendix itself. In the cases of localized abscess, we really need not operate in the presence of pus. The pus can be wiped away neatly and completely, so that we are not operating in the presence of pus any more.

When Dr. Knott read his paper on this subject I felt I had not the skill to do as Dr. Knott had done, and I anticipated trouble because I feared I should not be able to separate adhesions and get down to and deliver the appendix without soiling the general peritoneal cavity and lighting up a general peritonitis in a great many cases, but it was a very pleasant surprise to me to find that it is not so much Dr. Knott's skill that is responsible for his successes along this line, but it is the principle which he has advocated and defended which is responsible for his successes.

I feel I owe Dr. Knott a debt of gratitude. I believe he has taught me one of the best lessons of my life.

I have heard in this Western Surgical Association many papers which gave me ideas that I could take home and use in my work, but there is not one of which I have made better practical use than the one I received from Dr. Knott, and which he has given us a second time today in his paper.

Dr. Clifford U. Collins (Peoria, Ill.): I would like to ask Dr. Knott how long he waits after an attack is apparent before he decides to drain and remove the appendix? What is it that enables him to choose the time at which he does it?

I believe the appendix should be removed while the patient is in the hospital, but several years ago I adopted a different plan, which is rather a compromise. I confess I have been afraid to remove the appendix and drain the abscess at the same time when the pulse and temperature were high and infection still acute, so that when an abscess is apparent I give the patient a little nitrous oxide and oxygen, make a small stab-wound, and drain the pus at the outer edge of the abscess cavity. In two or three days the pulse and temperature become normal; the acuteness of the infection has passed, and then I wait seven days and through a rectus incision to the inner side of the stab-wound I remove the appendix. It can be removed safely then. The small drainage-wound is covered with adhesive rubber dam, and the pus remaining in the drainage tract has lost its virulency. It has lost its capacity to do any harm. My method, as I have said, is a compromise. As soon as an abscess is definitely walled

off, we give the patient a little nitrous oxide and oxygen, make a small stab-wound, drain the pus, and when the acuteness of the infection is over remove the appendix.

Dr. Miles F. Porter (Ft. Wayne, Ind.): I take occasion again to point out the danger of the right-rectus incision because of the possibility of secondary hemorrhage, resulting from infection. This may be avoided by making the incision at a distance from the epigastric vessels.

Dr. Leonard Freeman (Denver, Colo.): When Dr. Knott made his proposition of removing the appendix in all cases, I was very much afraid of it, and I did not do it and talked against it. In the last two years I am doing practically what he does, and certainly believe he is correct. I am sure that Dr. Knott will admit that there are cases in which this should not be done, as, for instance, in cases such as we have sometimes in Colorado, of people with bad lung tuberculosis who develop appendicitis. In them it would be dangerous to prolong the operation. In cases where patients have bad kidneys or bad lungs, or in which they have been too long starved, we may well make an exception, so that the rule should not be universal.

I wish to call attention to a little stunt in draining the pelvis, and, as Dr. Knott has so well said, one of the principal things in doing this operation is to thoroughly drain the pelvis. To introduce a rubber tube down into the pelvis among the coils of intestine, one must either push the rubber tube down into the pelvis, which is objectionable, as it catches on everything, or pass the hand down into the bottom of the pelvis to feel where the tube comes, and conduct the tube along the hand. I have found a much better plan to use one of the ordinary old-fashioned proctoscopes or sigmoidoscopes, whichever may occur to you. Such a proctoscope has a caliber of an inch, and is introduced with an obturator which, rounded on the end, slips down among the coils of intestine, and goes to the bottom of the pelvis. The obturator is taken out, and the rubber tube with a mesh of gauze is slipped through the proctoscope, and held there with forceps; and the proctoscope is then removed. This simplifies the procedure very much.

Dr. W. W. Grant (Denver, Colo.): Dr. Basham's paper deals with a class of cases that are still rather common, although not so common as those which are within the peritoneal cavity. The tender point in these cases where the appendix is wholly retrocecal, is not the McBurney point, but

is about two inches higher on a line at right angles directly from the umbilicus and from the McBurney point upward. In these cases it will be found this is the tender point; and it is also in these cases that we are more certain to have later the condition of subdiaphragmatic sepsis. The pus in these cases goes by way of least resistance, and the point of least resistance is the posterior colonic space upwards. In these cases we have an abscess that does not present the ordinary features of appendicitis with suppuration. Rupture into the peritoneal cavity is not common in these cases. You have more time, if necessary, to investigate, and delay is not so dangerous. Ultimately, however, the results are bad if operation is delayed too long.

In reference to the practice of Dr. Knott in removing the appendix in all cases: My rule has been for several years that whenever I can find the appendix in any gangrenous or suppurative state to remove it. I have not found the results any better by pursuing this course than formerly when I resorted to simple drainage in the abscess case; but I do find that the convalescence is shortened when the focus of infection is removed promptly. I do not believe you can ligate such an appendix and accomplish anything by it. Often, you cannot in these cases prevent a fecal fistula; and it does not, as a rule, require a secondary operation for its closure. These cases will generally close in a few weeks time without a secondary operation.

I distinctly differ with Dr. Knott as to his technic concerning the line of incision. There is another objection than that of hemorrhage, alluded to by Dr. Porter, and that is, in the case of an abscess posterior to the cecum and colon, why should we go through the free peritoneal non-infected cavity to reach and drain such abscesses? There is no necessity for it, and I consider it needlessly dangerous surgery. It seems to me that the practice of operating in the lumbar region external and posterior to the colon is best. By this method the appendix can be removed and abscess effectively drained without running any risk whatever of extravasation and general peritonitis. The existence of the latter can usually be determined before operation, and, of course, this would influence the operative technic.

Dr. Charles H. Wallace (St. Joseph, Mo.): I remember hearing Dr. Knott's first paper upon this subject, and I thought at that time it was a little radical; and at this time it may seem a little radical, but nevertheless progressive. I

believe that all of us, as our experience increases, do more complete work, and that every man who is actively operating does remove more appendices than he did formerly. Most of us seldom leave the appendix these days; yet there are cases in which I think Dr. Knott will admit that it is prudent not to remove it. Of course, these cases are rare, but occasionally they do occur. In speaking to a body like this, his teachings would have no harmful effect; but a two-step operation for the younger man is safer than the radical procedure that Dr. Knott recommends. I think I heard a prominent surgeon once say a man should never make the McBurney's incision, but should go through the right rectus. I never understood why. The abscesses very frequently in these cases that come ten days and two weeks after the initial attack, are at the outer edge of the abdomen. Nature has walled them off, and they are close to the ileum; and why should we go over and open into the general peritoneal cavity, instead of opening at the crest and tunneling under this and mopping out the abscess and removing the appendix? Why the other procedure is safer than this I have never been able to understand.

In most of these cases with the formation of abscess, I think we can remove the appendix without entering the general peritoneal cavity. Out of some thirty circumscribed abscess cases operated on by me in the last year, I lost but one. I believe I was responsible for the man's death. Being more confident in my skill than I should have been, I attempted to get the appendix out. It was an old case, one of three or four weeks standing; and the adhesions were dense. In getting the cecum up to remove the appendix a large rent was made in the ascending colon, and by reason of the inflammatory thickening I could not repair it, so I had to resect the gut; nothing else would do. The man died four days afterwards from general infection. I am satisfied that, had I drained this man and done a two-step operation, I would have saved his life.

In the few cases that I drain I ask that they return three or four weeks later for the removal of the appendix. I do not think any appendix should be left after a drainage operation, for the patient invariably has another attack. We have disproven by re-operation the former belief that the appendix sloughs and comes away with the pus. This rarely occurs, and only in those cases where the appendix is gangrenous and Nature envelopes it, and an abscess forms.

I do not agree with Dr. Knott that it is difficult to remove the appendix after secondary drainage. Nature places it near

the former scar. If you remove it early, before you get a chronic infection from repeated inflammatory processes, and firm adhesions are formed, it is a simple procedure to remove it.

As I understood the paper, it is dealing with localized abscess cases, and not those that have ruptured and emptied their contents into the general peritoneal cavity. In these I believe every surgeon realizes the importance of removing the appendix, removing the depot of infection, and thus giving the patient his only chance for life.

Dr. William T. Reynolds (Kansas City, Mo.): A number of years ago I awakened to the fact that there were two sides to a question. I do not want to feel bad towards the man who says it is not proper to remove the appendix in each case, but I want to say, personally, I think Dr. Knott's proposition on the removal of the appendix is as sound as a government bond. I had the pleasure of hearing his paper at Los Angeles, and also of briefly discussing it. I have listened to this discussion today, and find that it is tempered to some extent. There were a number of men from New York and Chicago who discussed Dr. Knott's paper at Los Angeles, and did so rather roughly. In fact, some of them would not listen to it at all when it came to the removal of the appendix, preferring to resort to drainage. As a matter of fact, it is not an exceedingly hard thing to remove the appendix at the primary operation. We must not fail to consider the fact that when a patient has a localized abscess that patient has undergone an autogenous vaccination. The patient is not ordinarily septic when the abscess is walled off. The patients who die from simple drainage, do not die from the want of drainage of the abscess that is opened, but they die from the want of drainage of an abscess that is not drained. It is the multiple abscesses, and these patients die from sepsis. Dr. Knott has proven in his series of cases that these patients do not die when all the abscesses are drained and the appendix removed.

I want to say a word or two with reference to the paper of Dr. Basham. The early symptoms of retrocecal appendicitis are not unlike those of ordinary appendicitis, and they are likely to lull the doctor into a sense of false security from the fact that the localization is not anterior but posterior.

If I may have the time, I would like to report a case of retrocecal appendicitis that occurred about four months ago in a little girl who was in school. A doctor was called. She had diffused bellyache. She was never well after that, but he

made a diagnosis of acute gastritis. She went home, and did not return to school. A little while later she fell into the hands of another practitioner, who discovered she had empyema. At this time I was called in consultation with the second man, and taking the history of the case into consideration I made a diagnosis of primary retrocecal appendicitis. It was found at operation that this girl had a subphrenic abscess plus a lung abscess as a result of retrocecal appendicitis.

Dr. George M. Gray (Kansas City, Kansas): In the first place, I want to congratulate Dr. Knott upon the report of this large number of cases with very few post-operative complications. It is certainly unusual to see so many cases as this reported with only one post-operative death from ileus. I presume the doctor will explain that by his method of drainage and the free liberation of adhesions. I want to confess that, in my experience, I have not been nearly so free from post-operative complications as the doctor.

One word in reference to Dr. Basham's paper on retrocecal appendicitis: It would seem that complications are somewhat more frequent in this form of appendicitis in so far as the liver is concerned. I have met in my own practice with two cases of multiple abscesses of the liver, both of which died. One was proven by autopsy. In the second case I was unable to get an autopsy, but the symptoms were so typical that there could be hardly any doubt.

The other case occurred two weeks after an acute appendicitis in which there had been apparently a perfect recovery, and the individual had gotten up and had gone about his business for two days when he was attacked by chills, followed by jaundice. The chills recurred at irregular intervals for the next two days up to the time of the operation, when a postcecal appendix was revealed with only a very slight abscess or slight collection of pus in the mesentery of the appendix. This individual went on for several weeks with irregular chills, and died. An autopsy was made, and showed numerous small abscesses all through the liver.

Dr. Charles E. Bowers (Wichita, Kansas): The idea is prevalent among a great many physicians that Dr. Knott removes the appendix in all of his cases at the primary operation. I would like to ask whether he does? As to localized abscess, it is not clear in my mind just what the doctor means. All abscesses are localized.

I would like to inquire whether he has reference to abscess without diffuse septic peritonitis.

I wish to report a case. I saw a case a few days ago with a temperature 100°, rapid pulse, very tense abdomen, and marked leucopenia. In this case I resorted to drainage through a stab-wound, and subsequently removed the appendix, with recovery.

Would Dr. Knott have operated on this case at this time, and if he did, would he remove the appendix at the primary operation?

Dr. James E. Moore (Minneapolis, Minn.): It seems like old times to have a discussion on appendicitis. I have been in the game from the start. I operated on the first case of appendicitis that was ever operated on in the state of Minnesota. I have been at it ever since. If all men were competent and knew their limitations, I would subscribe heartily to Dr. Knott's doctrine, but, unfortunately, we know that about the first thing the tyro surgeon undertakes is an operation for appendicitis abscess. The abscess has enabled him to make his diagnosis. I believe that this is very dangerous doctrine in the hands of such men. To the members of this Association it is a splendid doctrine, and I believe it is life-saving. The object of every surgical operation is presumably to restore to health the patient: it is not to take out the appendix.

Oftentimes it takes a wiser man to know when to quit than when to begin operating, and the tyros in surgery who operate on these cases of appendicitis do not know when to begin, and they surely do not know when to quit; and they fill graveyards in communities with patients whose lives might have been saved had they been content to have drained the abscesses.

I have changed my attitude in regard to appendicitis, I believe, forty times, and I may change it again; but I hope each time the change has been for the better. It has been based upon my own experience backed up by the experience of others. I may say that I very rarely, if ever, leave the appendix in cases such as were cited by Dr. Knott. I am very careful as a teacher of surgery to instruct these young men that the object of the operation is to save the patient's life, and not to take out the appendix. I tell them, when they see me operate on these cases in the clinic and remove the appendix, that it does not necessarily mean that they must do the same operation. I instruct the junior members of the staff to be more conservative than the older men are, and the consequence is we have a number of cases of this kind that are simply drained. They practically all recover, and we reoperate on them without waiting for the next attack.

Dr. Knott (closing): I wish to thank the members of the Association for the free and generous manner in which they have discussed my paper. I appreciate it very much. In my closing remarks I shall endeavor to be as brief as possible.

Dr. Collins asked as to the time of operation. I supposed that question was long ago settled, but I can answer Dr. Collins' question by saying that the time when I operate on a case of appendicitis with abscess is when I see it.

As to the remarks of Dr. Freeman, living as he does in a community like Denver, Colorado, where there are so many cases of pulmonary tuberculosis, perhaps complicated by acute suppuration of the appendix: One is justified in leaving the appendix in such instances. My experience in these 500 cases shows that I have never left the appendix yet. In these 500 cases that have been taken as they came—they have not been selected—I have seen no reason for leaving the appendix.

Dr. Grant referred to a fecal fistula as not needing a secondary operation. In many instances that is true, but the patient, nurse, and surgeon must regret the development of a fecal fistula in these cases, and it is a good thing to avoid, if possible.

In saying the incision should be made over the most prominent part of the swelling, and the abscess be opened extraperitoneally, the doctor is pursuing the same course he pursued at Los Angeles; that is, in the majority of cases of appendicitis he would leave the appendix because he thinks it is safer. In the majority of cases I think the appendix should be removed because it is safer to have it out, and the reason we make a right-rectus incision is simply that it permits the separation of all intestinal adhesions more freely than any other incision with which I am familiar or which we would be justified in using. It permits placing the drainage tube at the lowermost angle of the incision without an extra stab-wound for drainage and enables the surgeon to investigate the general condition of the peritoneal cavity. I contend that the peritoneal cavity may be opened with impunity at the outset as a deliberate step of the operation, and you can get into the peritoneal cavity and find out the condition, separate adhesions, and establish free drainage with safety. From a study of these 500 cases I should say that the danger of peritonitis following the separation of adhesions is remote. If that is so, if the bugaboo peritonitis has been removed, why permit adhesions to remain? Why permit many so-called secondary abscesses to remain when they can be removed at the first operation? Why permit multiple ab-

scesses to remain, or appear later necessitating a second or third or fourth operation, as has been the case in many instances.

Dr. Gray misunderstood me in one particular. I did not say there was only one case of post-operative obstruction. I said there was only one death from post-operative obstruction. Other cases of post-operative obstruction, complete and incomplete, have been noted, some operated on and some not, this being the only death from that cause.

My statement in regard to post-operative complications following this method was that the frequency of these complications is diminishing; the tendency to various forms of septicemia is lessened; and empyema, subphrenic abscesses, and other sequelæ do not occur in any such numbers as formerly.

Replying to Dr. Wallace, I will say that if Dr. Wallace had used the right-rectus incision in the cases he mentioned, he possibly would have gotten away without trauma to the colon.

With reference to what Dr. Porter said about hemorrhage, I will say that in this case the hemorrhage came from the epigastric artery following a right-rectus incision. The patient was allowed to bleed to death when any person with a small degree of knowledge could have controlled the hemorrhage. It was one of the unfortunate accidents encountered at times and rendered more tragic because preventable.

Replying to Dr. Basham's question, I will say that I remove the appendix in every case.

Dr. W. W. Grant (Denver, Colo.): Dr. Knott misquoted or misunderstood me in saying that I leave the appendix in the majority of suppurative cases. At Los Angeles I mentioned that in certain cases there were conditions, local and general, in which I did not believe it wise to break up adhesions and open the peritoneal cavity to search for a gangrenous appendix after the abscess was opened.

SURGICAL HANDLING OF THE PUS-INFECTED PERITONEUM

THOMAS C. WITHERSPOON, M. D.

BUTTE, MONTANA

After proposing the title of this paper, and while attempting to get into logical order that which I wished to present, I found myself somewhat in the predicament of the farmer who started off to town with a load of potatoes for market. The weather was bad, the roads rough, and he gave his entire attention to the way over which he traveled. Shortly, in a boggy stretch of country, he found himself mired. Turning to lighten the weight by dumping some of his load, he found, to his dismay, that nearly all the potatoes had been shaken out en route. "By Gosh!" he said, "This is tough. Stuck, and nothing to unload." My subject has been so thoroughly discussed, and the surgical literature is so full of it, there seems to be very little to add. It is a matter, however, to which I have given a good deal of attention, due to exceptionally opportune circumstances. My home in Montana is removed far from the larger surgical centers; and the usual urgency of many of these cases contributed to bring to my care a great deal of this surgery, which ordinarily is divided.

The time limit will not allow of an extended discussion of surgical care of the pus-infected peritoneum, but I wish to point out that which I believe practical.

Visiting various clinics where our representative surgeons handle such conditions, I have noticed quite a varied technic; and, while the literature is full of statements as to the best modes of procedure, such as, "handle the peritoneum as little as possible," "work rapidly," "drain well," etc., I find that many minor, even major, steps in operating are not carried out by all alike. It occurs to me there must be some "best method of procedure;" and the very fact that there is still a difference in the manner of dealing with these cases possibly justifies this further discussion.

The pus-infected peritoneum will include all cases in which pus is situated directly in the cavity, confined or free, and also those which necessitate a transperitoneal route for pus or infection.

It will be quite impossible to discuss at length the various lesions or injuries through the agency of which the peritoneum becomes infected, and likewise the technic of visceral repair or removal, except in so far as it concerns the matter of limiting the infection and producing the most desirable terminal results.

All pus-infections of the peritoneum come from a direct implantation on it or extension to it, of pus-producing organisms. Now and then we hear of a case in children of ideopathic streptococcic peritonitis, but I am sure you will agree with me that the error in such a conception usually lies in the fact that the port of entry has not been discovered.

The peritoneal cavity is most wonderfully capable of caring for itself when subjected to infection by a pus-forming organism, though the various organisms show different degrees of malignancy in the peritoneum, as they do in all other tissues of the body.

The first surgical necessity in the treatment of peritoneal pus, or infected peritoneum, is proper or adequate drainage. Notice that I do not say removal of pus or cleansing of the peritoneum. One of the

most noteworthy errors of a surgery just out of date is that of attempted cleansing of the peritoneum. We can all recall the tedious efforts made a few years ago to thoroughly douche out the generally infected cavity with every solution from a Thiersch to a normal salt; and in local pus collections, instead of a simple sponging supplemented by good drainage, we have seen the error of hydrogen peroxide, camphophenique, carbolic acid, alcohol, and iodine applications to sterilize.

One of the possible errors of modern surgery is that due to a fear of drainage; and now and again we hear advocated closure of the peritoneum, trusting to its faculty of caring for an infection.

The second surgical necessity must be to prevent the addition of further infecting material to the peritoneum by so dealing with the port of entry as to securely shut it off from the cavity or give it complete and independent drain. Thus we close a perforation of the alimentary tract, remove a diseased appendix or Fallopian tube, drain a gall-bladder or suppurating solid viscus, and exclude, if possible, from the peritoneal cavity the rupture in a urinary bladder. A simple drain without attention to the initial lesion may suffice, but not to a degree of reasonable security. To this I wish to refer later in connection with the history of a case of perforated stomach.

In all cases of pus-infection of the peritoneum, the above must of necessity be universally applicable, regardless of mechanic or microbic cause. But to this must be added the further generally recognized considerations: First, the manner of accomplishing the operation as to speed, gentleness of manipulation, and accuracy; second, the location and manner of making the original entry into the peritoneum; third, the manner of preventing mechanical spread of the infection, if it is not general; fourth, the absolute necessity of complete surgical cleanliness, as though the condi-

tion operated on was not due to infection; fifth, the location of the drain, as affecting, not only the immediate need of being sufficient, but the ultimate need of causing no aftermath in the way of compromised visceral functions; sixth, the time to best deal with these infections, for the nature of the infecting agent, its manner of invasion, and the condition of the patient must be considered; and, seventh, the position to be occupied by the patient when abed after the operative intervention, for purpose of drainage.

For the purpose of best reducing the subject to concrete and practical outlines, let me turn to the manner of handling the individual conditions as we meet them in practice. To this end, I will first briefly consider dealing with pelvic peritoneal pus.

The vast majority of pus-infected pelvic peritoneums are found in women, only the few resulting from tuberculosis, malignant tumor, or trauma occurring in men. The large majority of cases in women are tubal, the result of vagino-uterine infection. The smaller number are of the post-puerperal or abortive kind, and the very small remainder result from trauma, tuberculosis, or neoplasm.

Rarely will it be found necessary to repeat the efforts of Duhrsen, i. e., to attack the process through the vagina. Except in extreme cases, where drain is temporarily made for the purpose of combating exhausting intoxication until required strength will allow of an adbominal procedure, the vaginal route is never preferable.

The time factor enters into the consideration of two types of pelvic infection for two reasons: First. The acute gonococcal pus tube. It is wisest in these to await a natural drainage with possibly a functionate restoration. The majority of simple acute pus tubes are gonococcal in origin. I cannot agree with those who declare surgical intervention in these cases is

unnecessarily dangerous. My experience has been to the contrary, for I have found such as I have operated on quite free from even a suggestion of extension of the infection or apparent post-surgical danger. The object of waiting before resorting to surgical removal of the tubes is to simplify treatment and allow of restoration of function. A certain proportion of these cases do return to normal without the necessity of surgical intervention.

Second. The acute puerperal or post-abortive streptococcic infections. I am not sure that a diagnostic separation of the three modes (interstitial, lymphatic, and phlebitic) of invasion of this infection can be made, as a rule, though the surgical necessity in each is different, and the time for surgical intervention will likewise vary. Did we but know the acute interstitial at once, I doubt not that immediate hysterectomy per vaginam with a large drain would give the least mortality. This minimizes the area of peritoneal exposure, and offers the best possible route for discharge of inflammatory material. If a diagnosis of acute phlebitis of the utero-ovarian vessels could be arrived at early, no doubt an immediate ligation through a suprapubic incision would accomplish the best possible results. Section of the veins should always be made, and a gauze gutta-percha drain from behind either broad ligament serves as the best means of directing any discharge. But in case the parametritis and perimetritis is due to a lymphangitis, then to wait until a reasonable immunity has been established is always wise. In our hospital we have been gratified by a remarkably low mortality in these last-named cases, which, fortunately, constitute the majority of the post-abortion infections. In these, a tubal removal, which serves to drain the broad ligament, is most useful. The uterus should be sutured to the anterior wall at the lower end of the opening.

A median rubber tube into the cul-de-sac of Douglas, with two gauze gutta-percha drains posterior to the broad ligaments, gives exit to all secretions.

In all types of pus tubes operated on, it is wise to remove the whole tube, and not attempt to preserve function by a partial or so-called conservative salpingectomy. The broad ligament is whipped over, to control the bleeding and to close raw surfaces, with catgut starting at the uterus and doubling back to end at the uterus. Thus the knot of the necessarily infected gut is brought near to the place of exit of the drain through the abdominal wall. The uterus, after salpingectomy, is always fastened to the lower end of the abdominal wound after the method of Olshausen-Kelley. This allows of a better suprapubic drain from out the cul-de-sac of Douglas, and does not compromise bladder function.

It seems almost needless to say that the gauze sponge immediately introduced after opening the abdomen to pack the bowels back out of the field of operation, should keep them out of sight during the entire procedure, and serve to catch all infection which escapes during manipulation. I have seen various operators flush out the pelvis after spilling pus during the operation. This certainly is a dangerous custom. If the pus is old and sterile it can do no damage, and if infected certainly the presence of a solution only helps to diffuse the agent for further peritoneal involvement.

One of the commonest causes of general peritoneal pus is a perforated or gangrenated appendix. Only a few years ago these cases were nearly universally fatal, whereas the mortality now has been reduced to an exceedingly small per cent, and we are contending to reduce this to as nearly zero as possible. I have been interested in observing the manner of handling these cases; and I must say there seems to me a need

of some uniform method. First of all, why waste time, after exposing the appendix and cecum through a crisscross incision, by invaginating the stump? Many of these cecums are somewhat ptotic, and might to advantage be anchored. When the appendix is cut from the clamped meso-appendix, a linen thread should be tied tightly about its base before removing it. The ends of the ligature should then be threaded through a small rubber drainage-tube, which is carried down over the stump. This is very quickly accomplished, and offers perfect local drain. A catgut ligature controls hemorrhage from the appendix vessels, and the ends, left long, should be brought through the wound by the tube, increasing for a while the capillary drain. This may be supplemented by a small cigarette-drain, which never should be allowed to pass over the pelvis brim into the true pelvis; otherwise, one or more loops of small bowel may become obstructed by adhesion to the dense pelvic fascia along the course of the drain, and produce obstruction during the second or third week following the operation. The general cavity should be drained by means of a metal drainage-tube, such as I show here, introduced through a suprapubic puncture. I cannot agree with those who proclaim the danger of the solid drainage-tube at this site. It can be more readily and accurately introduced through the suprapubic puncture than can a rubber tube; and in my hands it has not caused fistula or other complication in somewhat over two hundred cases of general peritoneal pus. In this manner, general peritonitis from a diseased appendix may be operated on in a few minutes with the least possible handling and shock, and the best sort of drain.

I even prefer a large arm-chair to the Fowler position in bed, for a few days following, in order that the most advantageous position for drainage can be main-

tained comfortably. This procedure is least apt to be followed by post-operative hernia.

An exceedingly interesting class of cases are those in which a general peritonitis follows traumatic rupture of the bowel. A rapid closure of the rupture, which should be anchored to the wound, if possible, supplemented by a small gauze gutta-percha drain from beneath and brought out at the lower end of the incision, and a metal tube-drain through a suprapubic puncture, constitute the best surgical treatment. Here I warn against sponging and flushing of the cavity, even if soiled by feces.

Another interesting general peritoneal involvement is caused by perforation of an ulcer of the stomach or duodenum. A right rectus incision from the costal arch downward three or four inches, will give easy access to the visceral lesion. The ulcer should be closed transversely to the axis of the organ by, first, a linen stitch, and this, in turn, covered in by a continuous catgut stitch. A rubber tube should be anchored to the ulcer area by the catgut stitch, and brought out at the lower angle of the wound, in order that easy drain can be had in case of leakage. A suprapubic drain out of the pelvis, by means of the metal tube, constitutes the final step. Regardless of what might have escaped through the perforation from the stomach, I caution against a large lower abdominal opening for the purpose of flushing or cleansing the peritoneal cavity. A marked Fowler position afterward will take care of this.

On one occasion recently, I had a man brought to the hospital on the second day after such a perforation, with a pulse of 160, irregular and very weak. He was cyanotic, and all thought of operating under general anesthesia had to be discarded. Accordingly, I made a simple suprapubic puncture, and introduced the metal tube into the pelvic cavity under eucain

anesthesia. Twenty-four hours afterward he had improved remarkedly. At this time I was tempted to complete the operation under general anesthesia; but, as he was gaining, I decided to wait. Absolutely no diet and no water by mouth was ordered. On the fifth day he appeared to be normal; but the senior house surgeon by mistake allowed a liquid diet. Unfortunately, I did not see him, due to absence from the city, for twenty-four hours. At that time he had lapsed into a condition even worse than when first brought to the hospital. He died shortly. I have always considered it a mistake not to have completed the operation as soon as his condition justified the giving of a general anesthetic. Whenever a perforation has occurred into an open peritoneum I believe it unwise to perform a gastro-enterostomy. Such a patient should first be tided through the general peritonitis; and then, in a few weeks or months, the radical procedure may be safely done for the purpose of complete cure. I am not prepared to say, however, that a reasonable per cent of such cases may not be permanently cured by a dietary regime without gastro-enterostomy, a Rodman resection, or some such procedure.

In all general peritonitis cases, regardless of what might have initiated them, I have ordered water only by rectum for at least three days. It has been given intermittently, usually every two or three hours, and not in a single instance continuously. Plain water is used, and not a physiologic salt solution. I have never understood why salt water by mouth should lead to increased density of the body fluids with retarded elimination, and that, by the rectum, it should act in just the reverse manner. I have not found water more irritating than the saline solution.

There are many other interesting matters which should be brought out under this discussion; but, unfortunately, the limit of time put upon each paper prohibits this. Suffice it to say that always the simplest, speediest manner of dealing with peritoneal infection, providing competent and proper drainage, gives the best results.

BEST METHODS OF DISCOURAGING THE ACTIVITY OF INOPERABLE CANCER

A STUDY OF HEAT IN CANCER

James F. Percy, M. D.

GALESBURG, ILLINOIS

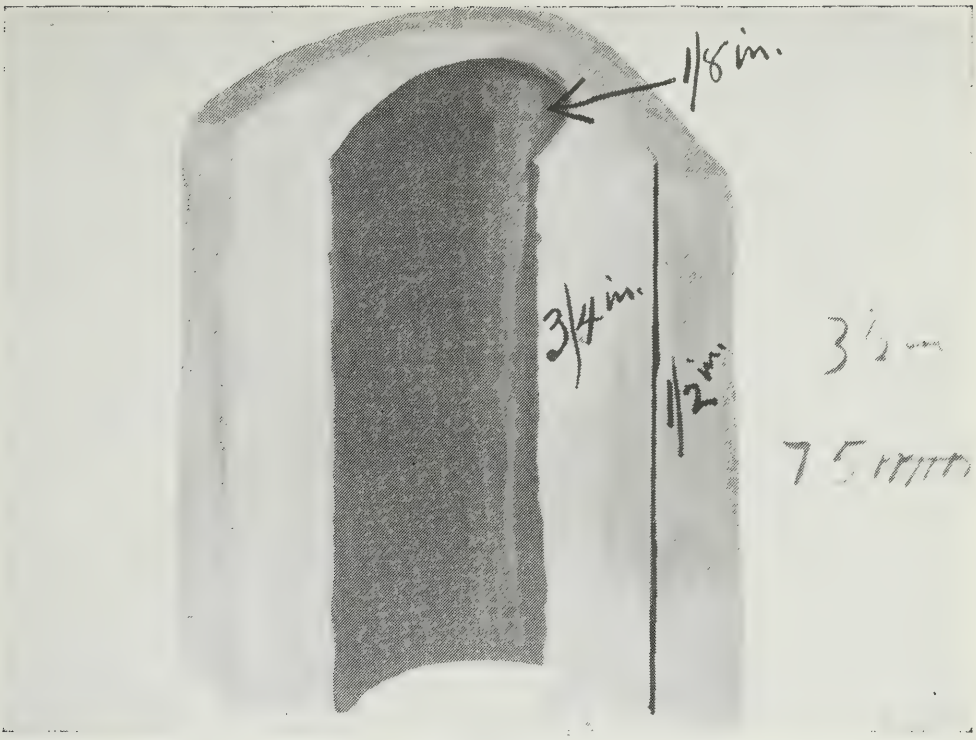
Among the earliest of the various procedures used in the treatment of malignant growths is the Chian turpentine advocated by Clay of Birmingham; there has been developed since then the molasses treatment; soap solution and ox-gall; cancroin, a poisonous product of cancer-tissue; thyroid extract; trypsin and amylopsin as strongly recommended by Beard; atoxyl by Skene Keith; Reicher's injection of epinephrin into the tissues about carcinomatous growth; Spiess, "337," an anesthetic not on the market, similar in chemical structure to novocain, by which Spiess obtained brilliant results in the carcinomas of mice, and Uhlenhuth's injection of pyocyanase. And now, among the most promising, at least from the laboratory point of view, are the selenium and tellurium compounds of eosin introduced by Wassermann and based on the fundamental work of Ehrlich. With these can also be considered the use of the colloids of certain metals, especially silver, copper, and gold, as described by Caspari, Neuberg, and Loeb. Under this general subdivision may also be placed serotherapy, namely, the

fetal autolysates of Fischera and emulsions of Levin and Babcock; organ extracts treated by the Röntgen ray and autoserotherapy as described by Hodenpyl and Ill. These methods have excited great interest from the point of view of the experimental laboratory. This has been especially true of the fetal autolysates of Fischera, which, from the scientific point of view at least, have much to commend them.

Among the rest may be mentioned the serum of animals treated with cancer by Jensen; autolysated sarcoma by Blumenthal; epinephrin and antivenin horse-serum of Greenbaum, and defibrinated blood by H. C. Ross and Bier. I purposely omit a bibliography as not being germane to the subject-matter, and would refer the reader to the status of carcinoma therapy as given in the *Reports of the American Association for the Study of Cancer,* 1913.

In spite of our wishes and hopes, all of these methods have proved to be inconstant in their effects, and therefore unreliable and disappointing. From the general agents used in attempts to destroy carcinoma, such as these just mentioned, we can pass to those employed locally. Among the first is probably arsenic, and its history is neither unimportant nor disappointing, although it has been badly used in many instances. With the arsenic, zinc chlorid, calcium carbid and acetone have also been employed.

The Röntgen ray has played an important part in the local treatment of cancer, and it is probable that it is destined to play a greater rôle if the work of Baum of Berlin and that of Kroenig and Gauss of Freiburg holds up their early promise. That the Röntgen ray will inhibit and destroy the carcinoma cell, or germ, under favorable conditions, there can be no reasonable doubt. There remains for the physicist only the problem of making the rays reach in an effective way the



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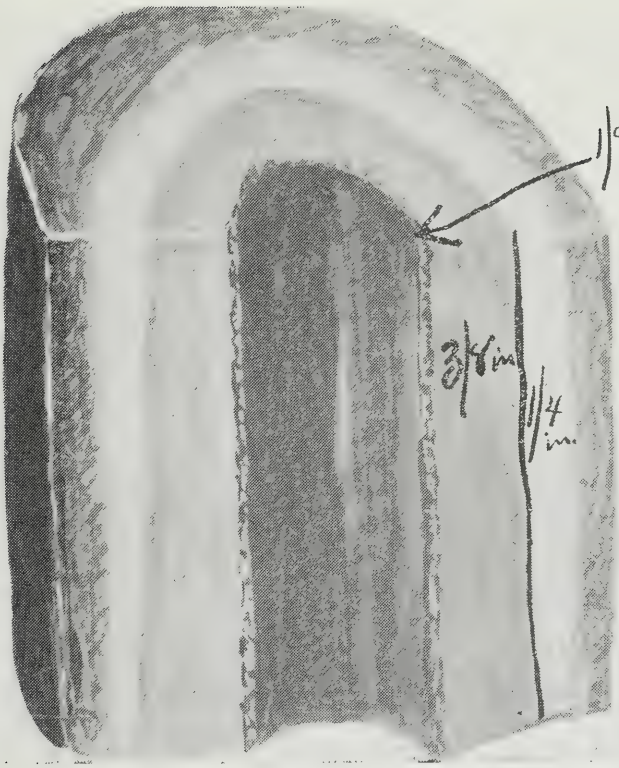

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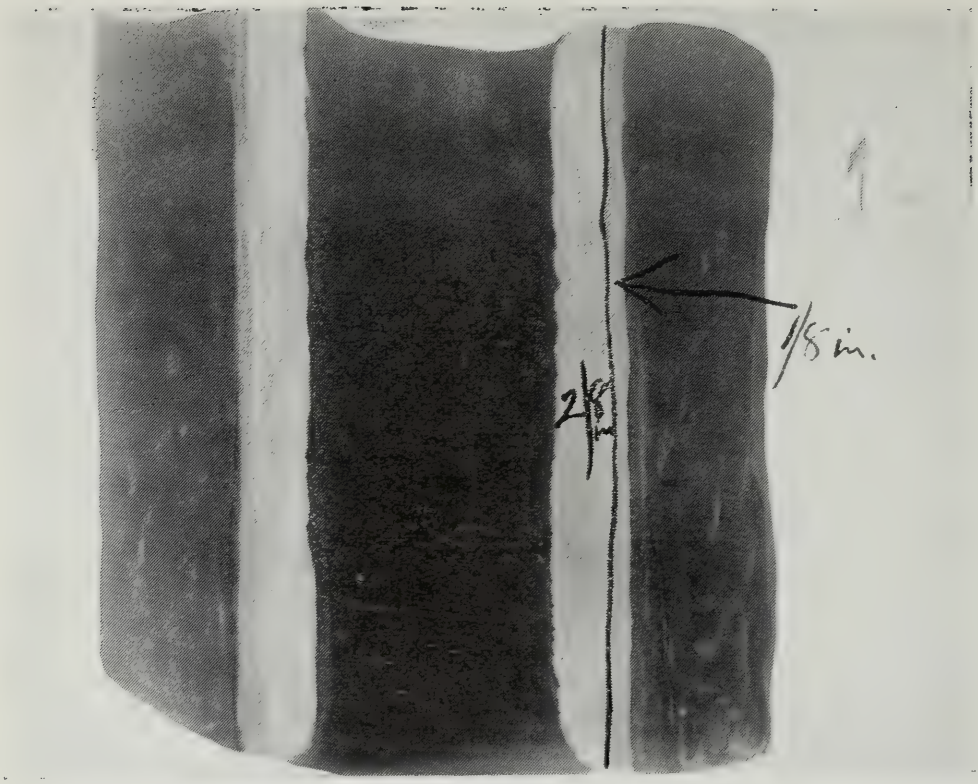

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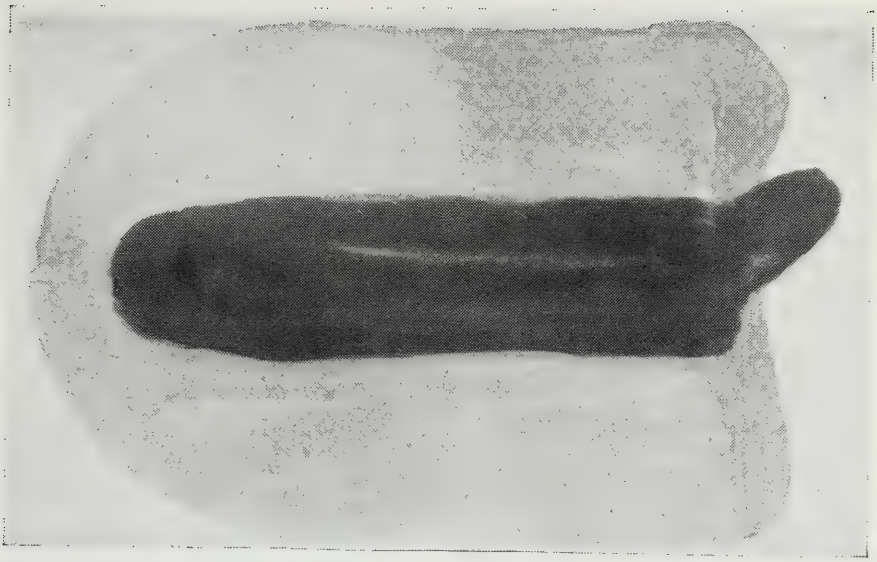

Plate 5.

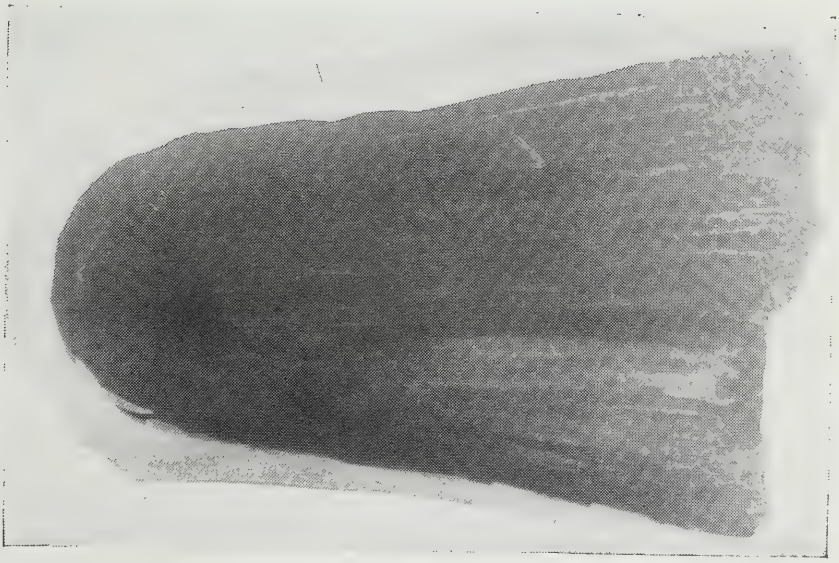

Plate 6.

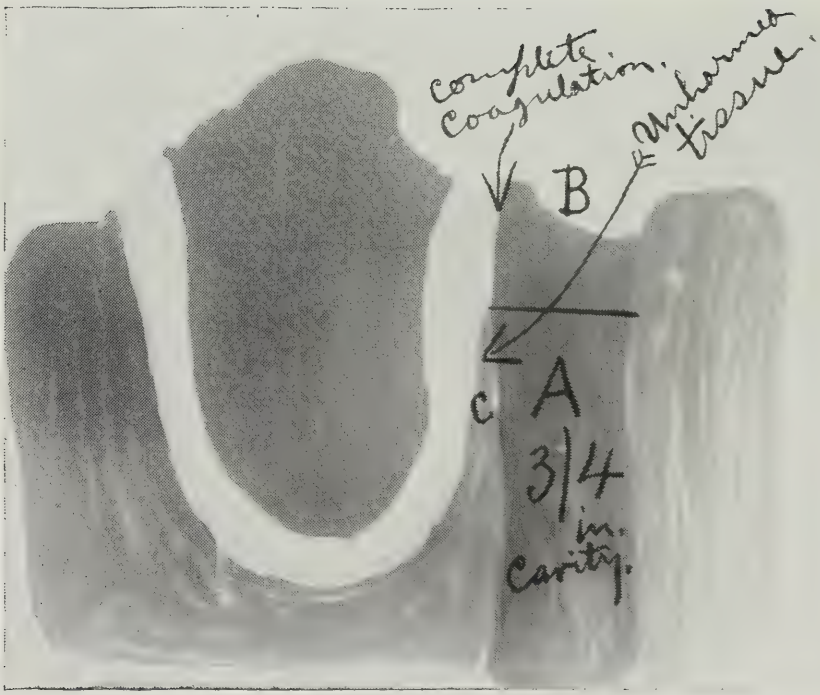

Plate 7. Complete coagulation. A. unharmed tissue.

most inaccessible regions of the body, the habitat of cancer.[1]

Radium also comes in this category. In reference to this agent, all that can be said at present is to express the hope that at least half the enthusiasm shown by the few who have had the rare opportunity of using it in large quantities will prove to be justified by subsequent results. Various modifications of the electric current also deserve recognition in the local treatment of cancer. With these, as with the Röntgen ray, the problem is also one of sufficient penetration to destroy the pathologic tissue, and leave, with the least amount of damage, the uninvolved tissue-cells. There are many of these methods. The most attractive, and, perhaps, the most promising, is the co-called fulguration method of de Keating-Hart, and also his thermoradiotherapy method. The diathermy system of Nagelschmidt and the electrocoagulation developed by Doyen of France belong to this same type. That these are scientific and based on good theoretical, as well as experimental data, there can be no question.

Finally, we have, as a last hope in the management of inoperable carcinoma, the operative treatment so well set forth by Mr. Cecil Rowntree.[2] The essence of this author's paper is the relief of suffering by the excision of the growth as far as possible, without too greatly endangering the life of the patient. Added to this is the expectation that death by metastasis will be more merciful than ulceration and septic absorption.

As already stated above, under the head of general methods, these various procedures, although of the highest scientific interest, are too indirect for consideration in a practical way at this time. In an effort to determine an agent, aside from surgical means, to eradicate carcinoma, numerous observations have been made on the vulnerability of cancer-cells to various physical and chemical agents. In 1905, Haaland,[3] in

attempting to obtain artificial purification of mixed tumors, following the suggestion of P. Ehrlich, showed the susceptibility of inoculable material to heat of 44° C. (111.2° F.) for half an hour. Ehrlich suggests that with higher temperatures, fewer and fewer cells survive. Haaland likewise showed that carcinoma-cells

Jensen[5] has placed the vulnerability of cancer-cells succumb before sarcoma. Clowes[4] demonstrated the death of all tumor-cells exposed to a temperature of 45° C. (113° F.) *in vitro,* and called attention to the fact that weak strains of tumors increase in virulence when incubated at from 38.5° to 41° C. (from 101.3° to 105.8° F.), while the opposite is true of strong strains.

at 47° C. (116.6° F.) for five minutes; Leob for sarcoma 45° C. for thirty minutes; Clowes and Baeslack 44° C. for thirty minutes.

Lambert[6] has recently pointed out that temperatures about 42° C. (107.6° F.) were distinctly harmful, and that the degree of injury to both normal and cancer-tissue depends on the degree of temperature and length of exposure. He further states that sarcoma-cells are destroyed by exposure to 42.5° C. (108.5° F.) for from twenty-four to forty-eight hours, 43° C. (109.4° F.) for six hours, 44° C. (111.2° F.) for fifty minutes and 46° C. (114.8° F.) for twenty minutes; while normal connective-tissue cells survive these various exposures.

M. Doyen, in proposing the *Micrococcus neoformans* as the cause of carcinoma, showed that the death-point of cancer-cells is from 55° to 56° C. (131° to 132.8° F.), identical with that of the *Micrococcus neoformans.*

E. Vidal[7] noted the arrested evolution of tumors in four patients with a temperature above 40° C. (104° F.). In order to be sure that there was not a quadruple coincidence, he daily exposed tumor-bearing mice to

temperatures above normal, and found that they lived longer and that the tumors were the seat of degenerative changes. In a bitch having a spontaneous lymphosarcoma, the zone of Richet was punctured, the temperature rose to 40.8° C. (105.3° F.) and the tumor diminished in size rapidly and finally disappeared some time after this procedure. In this connection Vidal makes the startling and original observation that the methods of treating carcinoma by vaccines and toxins, such as the nectrianin of Bra, Doyen's *Micrococcus neoformans* vaccine, toxins of Coley, vaccine of Otto Schmidt, etc., are all attended by a reaction, the principal symptom of which is an increase of temperature, and when such reaction is absent, the several authors state that there is no salutary influence on the carcinoma. He therefore concludes that these various methods owe their success largely to their fever-producing qualities. It may here be mentioned that Bier[8] has shown that the injection of a foreign defibrinated blood is attended by fever, and it is well known that the intravenous injections of colloidal metals show the same reaction. The various reports of diminution of malignant tumors following infections, such as erysipelas, assume a new significance.

Based on the foregoing experimental data, it is logical that we hope for the eradication of cancerous growths by attacking these morbid processes through their vulnerability to heat. It is immediately apparent that methods tending to raise the general temperature of the body above 104° F. (40° C.) are at times dangerous, always uncertain and beyond the power of regulation. It, therefore, remains to develop a method of applying heat to the tumor mass to a degree of efficiency that will permit of the greatest destruction of carcinoma-cells with a maximum conservation of normal tissue-cells. The methods which may be enumer-

ated to obtain these results are hot air, hot water, electrocoagulation, fulguration and actual cautery. The first two may be readily dismissed, because the penetration of hot water, as shown by Doyen,[9] is from but 4 to 5 mm., and that of hot air is probably much less. Electrocoagulation will produce a coagulation of tissues from 5 to 8 cm. in two minutes, when the electrode is in immediate contact with the tissues. The rapidity of this action, the inability to direct and control the electrode within the cavities, aside from the costly and complicated apparatus required, together with the refinement of technic and specialized judgment of the operator, make this method impracticable in the treatment of the cavity carcinoma.

In considering the de Keating-Hart method of fulguration, a method applicable to superficial growths after their surgical removal, it is not amiss to call attention to the work of Durig and Grau,[10] who show that the effects of the application of high-frequency currents is due to heat alone, and not to any specific action on the tissue-cells. In comparison with the difficulties of the application of the foregoing methods, I have devised a practical method for the application of heat in the treatment of carcinoma that has none of the objections that the various methods already enumerated have. The penetration of the heat by the method to be outlined can be definitely determined and regulated. Its applicability has almost no limitations, when the malignant process is at all accessible; the required apparatus is not expensive, and it is easily portable. The method to which I refer is the application of heat from an electrocautery, accurately controlled by a rheostat, and applied to the affected tissues.

In an effort to determine the degree of heat resulting from the application of the heating-irons for

varying periods of time, the following experiments were performed:

Experiment 1.—Into a mass of fresh, tightly bound beef 7 inches in diameter and 6 inches thick, the largest electric cautery was allowed to burn its way under pressure. Two thermometers were forced into the mass at 1 and 2 inches' distance from the cautery, respectively, and the cautery was allowed to act continuously at constant heat for seventy-five minutes. Cross-section of the mass following the experiment showed just outside the area of actually destroyed tissue (charcoal) an area of coagulated tissue ¾ inch thick, and to the outer side of this, an intermediate pale grayish-pink zone ½ inch thick. In every instance the latter area was found to be to the inner side of the thermometer, which registered 55° C. (131° F.) (See Plate 1.)

Experiment 2.—A similar cautery was forced into a mass of tightly bound beef-muscle measuring 3½ by 3½ by 3 inches, and allowed to act continuously at constant heat for sixty minutes, and with a thermometer placed in the mass at a distance of 1½ inches from the cautery. Cross-section of the mass then showed a coagulated zone ⅜ inch thick, and an intermediate zone ¼ inch thick. Thus there is seen the actual destruction of a mass of tissue measuring 2⅜ by 2⅜ by 3 inches. (See Plate 2.)

In both of these experiments, frequent thermometer readings showed an almost constant rise of 0.5 degree C. per minute in the tissue within from 1 to 2 inches of the cautery.

Experiment 3.—The cautery, heated to a bright cherry red, was allowed to act for ten minutes within a mass of tightly bound beef-muscle. At the end of this time, cross-section showed a coagulated zone ¼ inch wide, and an intermediate zone ⅛ inch wide. (See Plate 3.) This experiment illustrates most graphically the inhibition by the charcoal core of the dissemination of heat. High degrees of heat in the cautery head thus defeat the purpose of this technic, through the formation of an almost impenetrable carbon barrier. It is well to emphasize here the importance of Experiment 5, in which the tissues were not carbonized by high degrees of heat, with a resulting coagulation of the tissues throughout a far greater area.

Experiment 4.—This was the same as Experiment 3, with the cautery acting for thirty minutes. Cross-section then showed a coagulated zone of ¼ inch, and an intermediate zone of ¼ inch. (See Plate 4.)

In all of the foregoing investigations, it is to be emphasized that the cautery was inserted hot, and allowed to act under high heat.

Experiment 5.—From the center of a mass of tightly bound beef-muscle 2½ inches in diameter and 3 inches thick, a core ½ inch in diameter was bored out. Into this the cold cautery was inserted and allowed to remain at a heat too low to cause charring of the tissue, for sixty minutes. At the end of this time, the entire mass was hot, and cross-section showed complete coagulation throughout the mass.

Experiment 6.—In this experiment, a mass of beef 3 by 3 by 4 inches was used, the object being to determine the time necessary entirely to destroy this mass by the cautery heat, combined with external pressure by forcing the tissue on the cautery, in this way simulating the actual operation in my technic of applying heat to a carcinomatous uterus. The mass was held in the hands of an assistant, who exerted constant pressure toward the cautery acting at high heat within the mass. At the end of twenty-eight minutes, the entire lump was too hot to be held in the hands, and section showed nothing left but a charcoal shell surrounded by a rim of well-cooked coagulated tissue ⅜ inch in thickness. (See Plate 6.)

Experiment 7.—From a large mass of beef-muscle, a core ¾ inch in diameter was bored and lined with a rubber glove. The opening of this cavity was then closed by a cork containing an inlet and a smaller outlet. To the inlet was connected an irrigator containing cold water, which was allowed to flow through the cavity in the meat. The heated cautery was then plunged into the mass at a distance of ½ inch from the cavity in which the cold water was flowing, and allowed to act for thirty minutes.

The object of this investigation was to determine the effect of cold water within a cavity in protecting directly contiguous tissue from coagulation by the heat from the cautery. Cross-section of the mass showed complete coagulation of all the tissue between the

cautery and the cork inserted into the cavity, but a zone of unharmed tissue ⅛ inch wide along the edge of the water-cooled cavity. (See Plate 7.)

From the foregoing experiments several important facts may be deduced. The temperature within the mass of tissue, measured at a distance of from 1 to 2 inches from the outside of the heating-iron, rose 0.5 degree C. per minute, the rheostat reading high. This fact affords a method for the relative measurement of heat during the cautery operation. The time of exposure and reading of the rheostat being the factors considered in the calculation. It will be seen that a considerable area of tissue, far beyond the zone of coagulation, was heated to a degree sufficient to kill carcinoma-cells.

The penetration of heat into a mass is dependent on the following factors: (1) the density of the mass; (2) the compression of the mass on the cautery; (3) the degree of heat developed and length of exposure, and (4) the type of entrance made for the cautery.

Experiment 5 shows in a startling manner that if a charcoal core is avoided, the area of coagulation far exceeds in size that ensuing from the application of greater heat for a longer period of time. In this experiment the cross-section shows that without carbonization, coagulation is produced 2½ inches in all directions from the surface of the heating-iron, the heat being applied for sixty minutes. When the iron was heated to a degree at which carbonization was produced, as in Experiment 2, again for sixty minutes, coagulation was produced for only ⅜ inch, and an intermediate zone of ¼ inch, making a total penetration of the heat into the beef mass of only ⅝ inch, in contrast to Experiment 5, with a much lower degree of heat, of 2½ inches from each side of the heating surface.

This emphasizes the caution which I have advanced on former occasions, not to produce too great a heat in the heating-iron; it is far better to develop a heat below the degree of carbonization, and allow it to act for a long time. As can readily be seen from these experiments, there are two methods by which a carcinomatous mass may be destroyed: first, that illustrated in Experiment 6, in which, with the heating-iron at high temperature, the mass is burned up, every angle being pressed on the cautery head by an assistant, and second, that illustrated by Experiment 5, in which the tissue is not burned up, but destroyed by the production of heat below the point of carbonization. Both procedures have a place in the operation. When a small, circumscribed and well-defined growth, unattended by pelvic metastasis, is present, the first method may well be used, particularly as a preliminary measure. When, however, the pelvis is filled with the cancer growth, the first method should not be employed, because the primary carbonization, I repeat, prevents in a remarkable way the dissemination of heat necessary to effective treatment of the cancer mass. In other words, the successful treatment of cancer depends more and more on the question of heat, and less and less on the effects of cauterization. There is another important consideration, which should guide us in reference to the prevention of the formation of the charcoal core at the point of contact with the cautery, namely, drainage. The charcoal not only prevents the scattering of the heat, but also interferes with subsequent drainage, which is a most important factor in the successful management of these cases. When the drainage is not adequate, the patient is in danger from absorption of the products of large quantities of killed cancer-cells. Wassermann has already called attention to this in the destroyed tumors of mice. He has shown that if the destroyed

tumor in these animals was as large as, or larger than, a hazelnut, the animal died from intoxication from the lytic cancer-cells. If, however, the tumor was the size of a large pea, this result did not follow when it was destroyed. In two of my cases, I have observed intoxication of this character. In both of these cases the pelvis was full of secondary carcinoma following panhysterectomies. The heat was applied through the vagina, and in both instances there was a perfect primary recovery. After four days, however, each of these patients showed signs of failure, which was expressed by extreme weakness, which could not be explained by any local findings. There was also a rapid and weak heart-action, associated with syncopal attacks, vomiting, marked stupor and finally an acidosis. The second method is much more effective, carried out for a period of time varying with the size of the mass; the heat radiates from the heating-iron in a circumferential manner and penetrates the tissues as outlined in the foregoing experiments.

The scope of this paper and the tenor of its contents does not permit of a description of the various steps of my operation, the indications, contra-indications and the immediate, together with the final results. Permit me to refer to my two previous articles on this subject for these data.[11]

CONCLUSION

It has been my purpose to try to point out, through the medium of this paper, the widening of the field of investigation regarding the vulnerability of the cancer-cell to the effects of heat. It will be of particular interest to make further observations as to the relation of the reaction, producing heat in the body, following the injection of toxins, serums and vaccines in the destruction of carcinoma. If the primary gross mass of cancer which is usually accessible can be

rendered innocuous by raising the temperature through the medium of an electric heating-iron, and the remaining small amount of lymphatic involvement be reached by thermic-raising, artificially produced toxins, serums or vaccines, as is emphasized by Vidal, then the dream of the ages, on the part of the physician, of doing something with cancer, will be on the road to realization.

Finally, it becomes necessary to develop the application of heat by a method which is at once efficient and harmless, and I believe that I have done this by the technic which I have developed. I believe, further, that I have accomplished this in such a way as to make it highly probable that many forms of carcinomatous growths may be eradicated.

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DISCUSSION

Dr. A. W. Abbott (Minneapolis, Minn.): After Dr. Percy had very kindly shown me his method I adopted it in a few cases, and I want to report only one as answering two questions,—one in reference to the amount of heat, the other in reference to the amount of pain afterwards.

The patient was given half a grain of codeia three-quarters of an hour before the operation. Nitrous oxide was then given, instead of ether, to ascertain these conditions immediately after the operation and to make the period of anesthesia as short as possible. The heat was continued until the assistant stated that it was distinctly uncomfortable to his hand. One minute after the cautery was removed it was impossible to introduce the finger into the uterus on account of the heat; that is, to keep it there. That was sixty seconds after the operation, which shows the amount and continuance of the heat.

In reference to pain after this procedure: this particular patient having had only half a grain of codeia, and, having taken nitrous oxide and coming out of the anesthetic within less than a minute, at no time suffered any subsequent pain.

Dr. Emil G. Beck (Chicago, Ill.): The essayist has not had the opportunity to tell us the results, but I know from his previous communications that he has had satisfactory results, and now the question arises: How can he account for the local heat which extends only a certain distance to prevent recurrence in inoperable cancers where the disease has extended into the glands as far as the bifurcation of the aorta?

We have recently had in our hospital a case of this character, where my brother, Dr. Carl Beck, removed a cancer of the uterus with cautery, and I asked him particularly why he used that method instead of the knife, and he told me he thought there was less chance for recurrence. I do not know where he got the idea; probably it came to him by intuition. One of the most prominent surgeons in this country prefers the actual cautery when he divides the intestine in carcinoma, in order to sear the surface with the actual cautery so as to prevent recurrence. He remarked that fire was the best method of purification because the Bible already had advocated it. Undoubtedly Dr. Percy got the idea from medical literature that continuous graduated heat produces destruction

of the cancer cells, and spares the normal tissues, and therefore, if his theory and his experiments prove to be correct, it will give us a new line of thought by which we may eventually cure cancer. Of course, I can also see how in some of these cases harm might be produced by the excessive heat to neighboring organs, as, for instance, the bladder and the rectum.

My brother and I are very much interested in the use of radium in the treatment of cancer. In one case of cancer of the uterus operated on by him, he put a small capsule of radium, nineteen milligrams, into the wound, and left it over night. Although we have used radium in five cases of laryngeal carcinoma, we have no report to make as yet, but I think I can safely say we have seen some benefit from it. I can speak of the use of radium from personal experience. I had some on my own body. As soon as I read about the promising results in European clinics from the use of radium, I did not have any rest until I went to Pittsburg and secured a small quantity, and went immediately to Baltimore to have it tested. When I got into the Pullman car at night I did not know what to do with the radium as it was so valuable. I finally decided to take it into my berth and put it down at the bottom of my feet. It was a capsule within a silver capsule, and this was placed in a lead capsule three-eighths of an inch in diameter, and perfectly safe. As soon as I went to bed I could not sleep because my feet began to itch. I changed the radium from feet to my head; shortly afterwards my head began to itch. (Laughter.) I then told the porter to take charge of my satchel, and then I was afraid the porter might run away with the radium. (Renewed laughter.) A Baltimore gentleman told me that I should keep on changing the radium from pocket to pocket. When I visited Professor Halsted's clinic they put a gown on me, and I was constantly changing the radium from pocket to pocket underneath that gown until one of the gentlemen next to me asked whether there was anything the matter with me. (Laughter.)

Gentlemen, I think suggestion plays a great part in the action of radium. When a patient receives the radium treatment, it makes quite a difference to him or her as to how much radium he or she is getting, and how much it is worth. It makes quite a difference whether it is worth a thousand dollars or ten thousand dollars; and more particularly in the case of a woman who has, let us say, ten thousand dollars worth of radium in her uterus, is the mental impression to be considered.

Dr. William Seaman Bainbridge (by invitation), after congratulating the Association upon having listened to the excellent paper by Dr. Percy, spoke, in part, as follows:

"I have long been interested in the treatment of advanced cancer of the uterus, and in this connection I may mention, briefly, certain methods which I have employed.

"In the first place, I was interested in what has been said regarding the 'trypsin treatment,' or the 'enzyme treatment,' of cancer, as suggested by John Beard, of Edinburgh. After going over the theory personally with Beard, on several occasions, I decided to institute a thorough test of the method in connection with the work of the Research Department of the New York Skin and Cancer Hospital. We sent out letters of inquiry concerning the use of, and results with, the method to over six thousand surgeons in different parts of the world. The replies to these were so lacking in positive findings, and in uniformity of opinion, that I was still further stimulated to give the treatment such a thorough test as would serve to establish its value, negatively or positively, for all time.

"The test extended over three years, and applied to more than a hundred cases. All the requirements of Beard were met, the procedure being varied from time to time, to meet the changes in his views. Only one exception was made to the rule of following his directions, namely, that which was given in the course of the test, not at the beginning, with reference to refraining from operative procedure in any case subjected to the enzyme treatment. It goes without saying that I operated whenever I felt that the patient required an operation, or would be benefited by one. The report of this investigation, which was published in the *Medical Record,* and also in the First Scientific Report of the Research Department of the New York Skin and Cancer Hospital, announced absolutely negative results. Beard challenged the report in a book which subsequently appeared, but he brought out nothing new with reference to his theory, and nothing which nullified our findings. In other words, the negative report still stands, and there is nothing, it seems to me, to warrant the continued use of the method.

"A more satisfactory method of dealing with advanced cancer of the uterus, in my experience, is arterial ligation with lymphatic block. I have employed this procedure in over sixty cases, relieving suffering, controlling hemorrhage, and prolonging life, in some instances for months, and even for years.

"The technic, briefly described, consists in the ligation of the ovarian arteries, with double oöphorectomy,—the internal

iliacs, the uterine, the obturator, and the sacra media,—one or all, according to the exigencies of the case. In some instances, as a measure of necessity, the common iliac of one side may be ligated without untoward results.

"Either before or after this ligation operation lymphatic block is insured by the removal, *en masse,* if possible, of all the lymphatic glands, from the receptaculum chyli above to the obturator foramen below, on each side. The glands situated within and around the obturator foramen are removed. I have without difficulty removed the glands in this locality, taking them *en masse* and placing a hot pad over the area. If any of the glands are so softened that there is danger of breaking them and soiling the peritoneum, the operator must choose between the two evils,—leaving them early to break down themselves, or taking them out and running the risk of rupturing them, thus soiling the peritoneum. The danger of this contamination is slight if one is careful to pad off the rest of the peritoneum, and carefully to approximate the edges of the peritoneal wound, covering over the raw surfaces.

"In connection with this procedure, accompanying pathological conditions,—such as volvulus, 'kinks,' and other abnormal conditions of the intestine,—may be corrected as far as possible, and in sequence. The uterus may be extirpated in some cases where such a procedure seemed impossible before laparotomy. In many cases the victim of cancer may suffer coincidentally with non-malignant disease of the ovaries and tubes. Removal of these diseased structures may relieve the symptoms, the patient often having cancer, but not as yet suffering directly from it.

"With this procedure completed, the uterus may be curetted to a shell by the Byrne, the Percy, or any other method, without the danger of hemorrhage. Curettage and the adjuvant measures may be employed immediately after the ligation, or a week or ten days later, by which time the tissues will be much more contracted.

"In the Research Department of the New York Skin and Cancer Hospital we have been testing, also, certain physical agents in the treatment of cancer: (1 electrocoagulation (Doyen), (2) fulguration (de Keating-Hart), and (3) thermoradiotherapy (de Keating-Hart). I may say, with reference to these methods, that our application of them is of entirely too short duration to warrant any statement as to their curative value. A much greater length of time, a larger series of cases, a clearer knowledge of the action of electricity, how-

ever employed, upon normal and abnormal tissues, and more definite knowledge concerning the etiology of malignant growths, are all necessary, in order to estimate the efficacy of any method of treatment in these diseases. It must always be borne in mind that such methods of treatment may be fraught with harm as well as good, and their employment should be safeguarded in every way. The reports of cases which I have presented from time to time are given merely in the way of a report of progress in the testing of the methods. When our observations shall have been multiplied many times, and months shall have passed into years, a final report, from our point of view, may be given concerning the value of fulguration, electrocoagulation, and thermoradiotherapy in the treatment of cancer and allied diseases."

Dr. Percy (closing): It is necessary to answer some of the questions that have been asked in connection with my paper. Dr. Abbott speaks of no pain following this operation. That is absolutely true. It is quite remarkable that nearly everyone who has seen this method used is impressed with the belief that there must be an enormous amount of shock and great suffering following it, but there is absolutely none. The patients, when visited by the attendant the next morning, usually complain because they have not had breakfast.

In reference to Dr. Beck's remarks as to my results: It is too early to report results. I have a number of patients who have lived two or three years after, as Dr. Bainbridge has said, they were considered absolutely hopeless. I have one woman who was one of my worst cases, who was utterly devitalized from hemorrhage, who had an enormous carcinomatous cervix. I told her husband that she would probably die in three months, yet she is alive today, which is now over five years since the beginning of this treatment, and that was before my technic included the opening of the abdomen.

Another thing brought up by Dr. Beck that is very important is in reference to lymphatic extension of this disease. There is hardly any question that the pelvis protects the woman against the extension of cancer just as the pelvis protects a woman against the extension of infection from bad obstetrics. We know that the ordinary woman, as she is often taken care of in labor, by rights ought to die if there is anything in infection, as we ordinarily understand it.

Ries, of Chicago, says that sixty per cent of all cases of carcinoma of the cervix have lymphatic involvement in the pelvis, if you search for it. In opposition to this Wertheim,

Döderlin and Schauta all say it is not above twenty per cent. This is absolutely true in my own experience. Between these extremes, of course, the truth lies. Usually where I find enlarged glands along the iliacs or ureter, and have taken them out, and have had sections of them placed under the microscope, they have been reported as inflammatory, showing that the ulcerating mass in the cervix has caused an infective process that is not in the majority of cases carcinoma.

I mentioned trypsin because I got hold of an article by Beard in which he claimed that the reason why others were not getting results from his treatment was because they did not use a sufficient quantity of tryptic units. I do not know whether Dr. Bainbridge and his colleagues followed the directions of Beard closely or not in their work.

Dr. Bainbridge: We followed absolutely every direction given by him. We employed the method as he advised us, and our report in regard to the use of trypsin was final.

Dr. Percy: I am glad to know that. What I know also about fulguration is on the basis of what has been done with it in the treatment of papillomas of the bladder, and in this regard my experience also is necessarily limited.

SARCOMA OF THE OVARY

MILES F. PORTER, A. M., M. D.

FT. WAYNE, INDIANA

This paper is based upon a comprehensive, but not an exhaustive, study of the literature of the subject, including a study of 26 cases which I found reported in satisfactory detail, together with 3 cases occurring in the author's practice, making 29 in all. My cases reported in brief are as follows:

CASE 1.—Age 38. The present attack, consisting of abdominal pain, constipation, and bloating, came on five weeks before consulting me. Her menses, which had been regular, were overdue three weeks. She first noticed the tumor two weeks ago. It was the size of a seven-months pregnancy, nodular, and cystic. At operation a scar and a nodule were found on the colon. The abdomen was closed without removal of the growth. The patient was discharged one month later, unimproved. I could get no later word from her. No microscopic examination was made of the growth.

CASE 2.—The patient was aged 15. She had known of the tumor for one year, and had complained of pain in the right chest. She was very much emaciated, and the abdomen was larger than a pregnancy at term. She had never menstruated. I could not make out the cervix or os by a vaginal examination. There was some ascites. The diagnosis was a sarcomatous abdominal tumor, probably ovarian. At operation many adhesions were encountered. The bulk of the tumor was reduced by opening the larger cysts. It was not until after the mass was removed that it was found that it included the adnexa and the uterus, and that the sarcoma involved both ovaries. The solid portion of the tumor weighed nine pounds.

The patient made a satisfactory recovery, and was in perfect health six months later with no signs of recurrence.

Miscroscopic diagnosis: large round-celled sarcoma.

Case 3.—Patient aged 18, single. Her general health was good. She had noticed an abdominal growth for a few weeks before consulting me. There was no pain or other symptoms. A diagnosis of tumor of the left ovary was made. At operation the tumor was found to be solid, the size of an adult's head, and free from adhesions. On miscroscopic examination the tumor was found to be a fibrosarcoma. It is now twenty years since the operation, and the patient is well. Since the operation, the patient has married, and is now the mother of several children.

There is good reason for believing that many sarcomas of the ovary have been overlooked in the past because of incomplete microscopic study, and that, for the same reason, many tumors of the ovary were classed as sarcoma which in reality were not sarcomas at all. On no other ground can we account for the opinion of Gibson,[1] that sarcomas of the ovary are of uncertain malignancy, or for the opinion of Pfannensteil, quoted by Gibson, that removal of ovarian sarcomas is followed by 50 per cent of cures. According to Gibson (*loc. cit.*), ovarian sarcoma occurs once in a hundred cases of ovarian tumor. Schroeder (quoted by Adami and Nicholls) says sarcomata constitute 1.5 per cent of all ovarian tumors.

Wermuth[2] quotes Heine's table of 827 tumors of the ovary, 3.87 per cent of which were sarcoma.

Hartz[3] quotes Scharlieb as reporting only 4 sarcomata in 25 cases of malignant ovarian growths; and Norris as having found no sarcoma in 63 cases of ovarian growths, 10 of which were malignant.

According to Kelly and Noble, 50 per cent of ovarian tumors occurring in patients under fifty years, are sarcoma.

Sutton[4] says that the majority of solid tumors of the ovary are sarcomas.

W. Lippert[5] found 16 sarcomata in 638 tumors of the ovary (2.51 per cent).

Lockhardt,[6] in 65 cases of ovarian tumor, found 14 malignant, 3 sarcomata, and 11 carcinomata.

In 49 consecutive cases of ovarian tumor in my own practice, I found 2 sarcomata (4+ per cent). Case 3 is not included in this list, because it occurred twenty years ago, and it was impossible to make a consecutive list extending back very far. In 2,995 cases of ovarian tumor collected from the literature and embodying the work of thirteen observers, I found the incidence of sarcoma to be 5.43 per cent. Sutton[7] collected from current literature 100 cases of ovariotomy in girls under fifteen years of age, of which 21 were sarcomata. Sutton thinks, however, that this is far short of the real proportion of sarcomata because of the many cases in which no operation is done and the character of the tumors discovered at post-mortem. Averaging the percentages obtained from sixteen observers, and covering over 3,000 cases of ovarian tumors, we find the percentage of sarcoma to all other tumors to be 5.08.

In view of the foregoing, and taking into consideration a number of statements of authors and a number of tables concerning malignant tumors of the ovary to which we do not deem it wise to give definite and detailed reference here, the conclusion seems warranted that about 20 per cent of all ovarian tumors are malignant, and about 5 per cent of all ovarian tumors are sarcomata.

Contrary to the rule, sarcomata of the ovary frequently involve both organs. The only form of sarcoma which can be compared with ovarian sarcoma in this respect is glioma of the retina.

In the 29 cases collected by me the trouble was bilateral in four (13.78 per cent). In Wermuth's collection of 197 cases, both ovaries were involved in 26.9 per cent. Lippert found 2 bilateral sarcomas in 16 cases (12.50 per cent).

Averaging these percentages we concluded that sarcoma of the ovary is bilateral in about 17 per cent of all cases. We are therefore warranted in concluding that Sutton was nearly right when he said that both ovaries were involved in "the proportion of about 20 per cent of the cases."

Sarcoma of the ovary is especially likely to occur in the two extremes of life, although it may occur at any age. Doran[4] reports a case in a fetus of seven months, and Hoffman[7] reports an operation on a child of thirty-three months for sarcoma of the ovary. It would seem that all paired organs are particularly prone to sarcoma early in life. In my table the oldest patient was sixty-two, and the youngest ten, years of age. The average was thirty-five years. Three out of 28 cases occurred before the sixteenth year, 8 in the fifth decade or later, and 17 between the ages of fifteen and fifty.

Sarcomata of the ovary are usually of rapid growth. Doran's case, already referred to, was that of a seven months' fetus, in which the tumor was the size of an orange. In many cases the tumor reaches the size of a seven months' pregnancy within six weeks. In Steinharter's[8] case the belly was found filled with new growths within three weeks, and in two cases in my table the tumor reached to or nearly to the umbilicus in a month. Not infrequently cases are reported wherein tumors have been known to exist for months without increase in size, when suddenly they take on rapid growth. This sort of history indicates a tumor primarily benign which has undergone a malignant change.

On palpation the majority of ovarian sarcomata convey to the examining hand the sense of solidity and firmness, rather than a sense of elasticity and fluctuation, such as is conveyed in ordinary ovarian or parovarian cysts.

In 27 cases in my table in which this point is noted, the tumor was reported "hard" or "solid" in 13, and cystic in 4, and the remaining 10 cases were designated as "pultaceous," "brain-like," "firm and elastic," or "cystic and solid."

Concerning the complications met with in sarcoma of the ovary, it may be said that while there are none which are peculiar to this class of tumors, they are, on the other hand, subject to the same complications as are other tumors of the ovary. Ascites is the rule in both sarcoma and carcinoma, and does not occur frequently in benign tumors of the ovary. The ascites is present whether the malignancy involves the peritoneum or not. It may be said then that ascites in connection with ovarian tumor indicates malignancy, but not the character of the malignancy, and that the presence of ascites does not mean that the disease involves the peritoneum. Two cases in my list were complicated by pregnancy, one reported by Bircher[9] came to successful operation in the fourth month, and in one, reported my Maskus[10] Cesarean section was done for eclampsia. In the latter case there were bilateral melanosarcoma of the ovaries and carcinoma of the liver. In two cases there was a twist of the pedicle.

Winternitz[11] reports a case operated on for brain tumor, which came to autopsy, when it was found that the brain tumor was secondary to a melanosarcoma of the ovary.

Lippert, whose paper has already been referred to, found, in 168 cases, adhesions in 7, ascites in 11, sarcoma of uterus in 1, parovarian cyst in 1, fibromyoma of uterus in 1, and metastasis in 4.

In this connection it should be remembered, as pointed out by Sutton, that in dermoids of the ovary masses of tissue are often found which cannot be distinguished from sarcomatous tissue, and that the removal of such dermoids is sometimes followed by pelvic sar-

comata. Pucher[12] reports a case of giant-celled sarcoma developing in the wall of an ovarian cyst.

The co-existence of sarcoma and carcinoma in the ovary is rare. Cullen[13] reported one case two years ago, and says he could find no other case reported at that time. However, in an editorial note, in *The Journal of A. M. A.*, vol. lii, 1909, April 24, p. 1335, Forssner is credited with adding two new cases of carcinosarcoma of the ovaries to the small number previously reported, and with the statement that in all only eight or ten such cases had been reported at that time. Evidently, Cullen was not acquainted with Forssner's report when he made his. One would not expect carcinosarcoma of the ovary to be so rare, in view of the results of the experiments of Bashford and Haaland[14], who succeeded, by repeated transplantations of adenocarcinoma, in producing pure sarcoma; and in view of the fact that dermoids are potentially both cancer and sarcoma. LeConte[15], in discussing Gibbon's case, says he never saw a pure carcinoma of the ovary; but it does not appear in his remarks whether he refers to a combination of carcinoma with sarcoma. It is rather significant in this connection also that in the 29 cases tabulated for this paper, there was found co-existing cancer in other organs in two, one reported by Markus, referred to above, of cancer of the liver, and one of cancer of the pylorus, reported by Bushnell and Kewawalla[16].

All varieties of sarcoma have been found in the ovaries. According to Winternitz, whose paper has been referred to above, his was the fourth case of primary melanosarcoma recorded. In my search I found in 23 cases in which the character of the tumor is given, 2 melanosarcomas, 8 round-cell, 10 spindle-celled or fibrosarcomas, and three endo- or mesotheliomas.

The age of Winternitz's patient with melanosarcoma was twenty-six years. The age in the Markus' case is not given.

The average age for the round-celled cases in my table is a little less than twenty-six years, for the spindle-celled, a little more than forty-one years, and for the endotheliomata forty years. This age incidence, as it comes out in this table, is extremely interesting and suggestive, though, of course, by no means conclusive, and is corroborative of the consensus of opinion that round-celled sarcomata are more common in the young, and that spindle-celled sarcomata are more common in adults. The spindle-celled variety is the most frequent.

Sutton holds that malignant tumors in the ovaries of children are histologically distinct from the common forms of cancer and sarcoma, and should be grouped by themselves. He suggests that they be called *oöphoromata.*

The difficulty of correctly classifying malignant tumors of the ovary is very great. Steinharter *(vide supra)* accounts for this on the ground of the inherent metaplastic nature of the mesothelial cells. These cells, he says, are the least stable of the thelia. Adami[17], speaking of endothelioma says, "If of slow growth, such a tumor may present all the characteristics of cancer, and, if actively vegetative, be indistinguishable from sarcoma. Moreover, an intermediate type is frequently met with in which one part of the growth shows the cancerous type of structure, and the other part has taken on a hyaline type." In speaking of Ribbert's expressed doubt of the existence of tumors arising from the endothelium, Adami[18] also says, "It should be remarked in this connection that the so-called endotheliomas may in some cases arise from 'rests' derived from the mesothelial lining of

the celom, a possibility that does not seem to have suggested itself to the systematic writers."

The diagnosis of ovarian sarcoma is seldom made, save at operation or post-mortem.

Of the 29 cases collected for this paper, the diagnosis of "probable sarcoma" was made in 1[19], of "solid ovarian tumor probably fibroid or sarcoma" in 1[20], of sarcomatous abdominal tumor origin probably in ovary in 1. (Case 2 herewith reported.)

It would seem that the diagnosis might be oftener made if each case were carefully studied in the light of the facts now before us. Ovarian tumors occurring in girls under fifteen are quite likely to be sarcomata; and, if bilateral, it is highly probable that they are. Bilateral solid ovarian tumors in children are usually sarcomata. Great rapidity of growth should also lead one to suspect sarcoma, as should also rapid growth of a tumor that had been stationary for a long time.

Pain was a prominent symptom in ten of the twenty-nine cases collected by me, and would therefore seem to be a symptom of some diagnostic value. Either amenorrhea, menorrhagia, or metrorrhagia is much more common in sarcoma than in benign tumors of the ovary. One patient, aged thirty years, had amenorrhea for sixteen years, and then began to flow continuously. One patient, aged fifteen, had never menstruated. Two patients had a bloody vaginal discharge, which came on years after the menopause.

Any process which destroys completely the ovarian tissue will, of course, cause amenorrhea. A malignant tumor is more likely to destroy the tissue than a benign one; moreover, sarcomata of the ovary are peculiarly apt to be bilateral and to occur in the young, hence one would expect to find amenorrhea occurring with sarcoma more often than with any other form of ovarian tumor. One of my patients was fifteen years

old, and had never menstruated. This patient's menstrual suppression may have been due to other causes; but, inasmuch as she was not immature for her age and had what seemed to be a complete destruction of both ovaries from the tumor growth, I am inclined to attribute the amenorrhea to the tumors. Another patient had passed her regular period three weeks when she consulted me, and had not menstruated when heard from a month later.

Pfannensteil, quoted in Veit's "Handbuch der Gynecologie," says that amenorrhea with ovarian tumors is rare, but is more likely to occur in connection with carcinomata, sarcomata, and papillary cystadenomata when they are bilateral. All in all, he says amenorrhea occurs in about 2 or 3 per cent of ovarian tumors. Wiener[21] reports a case of amenorrhea with bilateral dermoids. As Wiener does not say anything about a microscopic study of the specimens from his patient, and as the case was reported only seven months after the operation and in view of the fact, stated above, that many dermoids contain tissue that cannot be differentiated from sarcoma tissue and that the removal of such dermoids is sometimes followed by recurrent growth in the pelvis, we are warranted in entertaining the suspicion that a careful search would have shown sarcoma tissue in the specimens removed from his case.

Wermuth says that sarcoma in the young may cause premature signs of puberty. I did not find any other author who confirmed this statement. That sarcoma of the ovary might cause a premature menstrual flow seems quite probable; but that it would cause developmental signs of puberty seems problematical.

Unless relieved by treatment, sarcoma of the ovary always terminates fatally. The prognosis is less favorable in children than in adults. Death occurs

from involvement of other organs by metastasis and by implantation.

The mortality of the operation *per se* is also much higher in children than in adults. Seven out of 21 in Sutton's collection of cases under fifteen years of age died from the operation, and 4 of the 14 that recovered died from recurrence within a year. The prognosis is best in fibrosarcoma; and, as in malignant tumors generally, the prognosis in all the forms depends, in large degree, upon the time of recognition of the trouble and the time of operation. Jones[22], of Providence, reports a successful operation for fibrosarcoma of the ovary in a woman fifty years old, who consulted him for vaginal hemorrhage; and he remarks that the prognosis is "good for several years in this type of case." In my table there were 3 operative deaths in 28 cases (about 10 per cent), excluding Winternitz's case which was operated on for brain tumor; and a case reported by Markus, who died of puerperal eclampsia for which Cesarean section was done. Lippert's mortality was 18¾ per cent. In 4 of Lippert's cases, however, metastasis was found when the operation was done. Considering the age, there seems to be no reason why the operative mortality in sarcomatous tumors of the ovary should be greater than that for benign tumors, provided there be no implication of other organs by the disease. It is well to emphasize the fact that, in those cases in which the operative mortality is high, it is so because of implication of other organs by the growth, either from implantation or metastasis, and that even without operation these patients could live but a short time.

In my table there are three cases remaining well for three years or more. (One for 3, one for 4, and one for 20 years.) This would indicate a permanent cure from operation in about 10 per cent of all operated cases.

Operation is advisable even in those cases in which the chances for success seem almost *nil.* In one of my cases (Case 2) the tumor was nearly as large as was the patient. After removal of the tumor the patient was extremely emaciated, and otherwise seemed an almost hopeless risk; and yet she recovered promptly from the operation, and is now (six months later) in perfect health.

Coe and Coley[23] report a case which at first was considered inoperable, but after giving toxins it was operated on successfully, and the patient lived for two years, dying from metastasis.

Seeligman[24] reports the successful treatment of a case of recurrent sarcoma of the ovary with metastasis in the spinal column by arsacetin intravenously and Röntgen rays. Bad effects are avoided by using small doses of arsacetin, and testing the color-sense every eight or ten days. Any disease of the eyes contra-indicates the use of the drug.

In conclusion, we may say that early operation in sarcoma of the ovary offers a good chance for permanent cure; that late operation cures rarely, but usually gives relief and prolongs life; and that some seemingly hopeless cases have been benefited and cured, for some months at least, by operation and the use of Coley's fluid, and by the combined use of the Röntgen rays and arsacetin.

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DISCUSSION

Dr. W. D. Haines (Cincinnati, Ohio): I want to relate the case of a girl, seventeen years of age, who had ceased to menstruate, and it was supposed by the family physician, and by the family, that she was pregnant. This condition was permitted to go on for an entire year; and as she was not delivered the idea of her being pregnant was questioned at least by the family doctor. At the time I saw her she was in fairly good health, that is, the blood-picture was good, she was eating well, and the bowels moving, but still her abdomen was greatly enlarged. Her abdomen was larger than that of a woman at full term. I told the family physician that I believed we had a malignant growth to deal with, and as we could feel it readily springing from the right side it was either from the connective tissue of the right broad ligament or from the ovary on that side.

I advised operation. It was accepted, and I removed the largest solid tumor I have ever taken out of the abdomen. I do not know the weight of it. The operation was easy. An incision was made from almost the xyphoid to the symphysis in order to get it out, and it was as easily done almost as the taking out of a partially cystic ovary the size of a fist. She made a nice recovery, and was put upon Coley's serum at once. The serum was not used as faithfully as I had used it in other cases, but no good effects followed the use of the

serum. She was operated on in December, two and a half years ago, and died the past August of general metastases throughout the abdominal viscera.

Dr. Porter (closing): Even after operation, as a result of pathologic study, these tumors are often taken for something else. I would suggest the advisability of careful and extended microscopic study of these tumors after removal. This has not been the rule in the past, as is proved, I think, by the statement of Gibson that sarcomas of the ovary are of uncertain malignancy, and the statement of Pfannenstiel, quoted by Gibson, that the removal of ovarian sarcoma is followed by 50 per cent of cures. These opinions can be explained only on the basis of incorrect diagnosis.

NON-PAPILLARY BENIGN TUMORS OF THE BLADDER

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Tumors of the bladder are classified into three main groups: (1) epithelial tumors; (2) connective-tissue tumors; and (3) muscular-tissue tumors.

The first group (epithelial) comprises the carcinomas, papillomas, adenomas, and cysts. The first two of this group are by far the most common tumors of the bladder, making up about 90 per cent of the total.

The second group (connective-tissue) contains the sarcomas, fibromas and angiomas. The sarcomas are occasionally seen in children, though these tumors are rare.

The third group (muscular-tissue) contains those tumors which spring from the musculature of the wall of the bladder, and make up between one and two per cent of all specimens.

Of these vesical neoplasms, including all of these groups, we have seen 164 cases. Two of these were of the non-papillary benign type which spring from the muscular layer of the bladder. It is the tumors of this group I wish to report at this time.

REVIEW OF LITERATURE

In a fairly extensive review of the literature, we have found about 30 cases reported.

Watson[1] reviews 653 collected cases of tumors of the bladder. Twenty of these were myomas. In 16 of these cases the tumors were removed by suprapubic operations, not resections; 13 of these patients survived; 5 are reported as having recurrences. Three were free from recurrence less than one year. Three were free from recurrence more than one year. Four was operated on by suprapubic bladder-resections; 3 survived. There were 2 reported recurrences; 1 survived six years. This report would seem to indicate that these tumors are malignant, or at least recur locally, and in that respect simulate the common tumors of the bladder of the epithelial type. In most of the other reported cases the patients have remained well indefinitely. Both of our patients are well now, over two years, without any signs of recurrence, and we did not believe from the microscopic and histologic examinations that these tumors should be called malignant.

A single similar case is reported by Gotzl[2]. He states that of 26 reported cases he found only 2 had had a probable diagnosis before operation. His patient had four small tumors situated near the urethral orifice. These were covered by intact mucous membrane.

Mandelbaum[3] makes the statements that myomas are not common, that they may be single or multiple; that they are covered by normal mucous membrane; that they arise directly from the muscle fibers of the unstriped variety, but that rhabdomyomas have been seen.

Davis[4] mentions one case in a series of 41 tumors of the bladder.

Keyes[5], referring to the pathology of tumors of the bladder, says that myoma is usually benign and that the tumor may be intravesical, interstitial, or subserous, and, like vesical fibroma, may pass unnoticed

unless it interferes with the mechanism of urination.

Blum[6] reports a case of a young man whose symptoms were severe bleeding and considerably increased temperature. An edematous tumor, the size of the fist and with a twisted pedicle, was found in the posterior wall of the bladder. He says the submucous type is the more common.

Bohme[7] reports one case operated on, and that the patient remained well after operation.

In most of the reported cases, as in our two, bleeding was the first and most marked sign in spite of the fact that no distinct ulceration of the mucosa of the bladder was found. The bleeding we presumed came from the congested mucous membrane of the entire bladder, especially about the tumor.

Starting in the muscular layer, these tumors may extend into the bladder and outward into the peritoneal cavity. They may attain considerable size, as in one of our cases the tumor could be palpated above the pubic bone, and was as large as an ordinarily distended bladder. Because of the rarity of these cases we have had much difficulty in finding detailed descriptions in the literature. The deductions, therefore, have been largely drawn from our two cases.

The location of the point of origin in the wall of the bladder in our two cases was close to the meatus of the urethra just above the left lobe of an enlarged prostate in one case, and about one inch above the urethra in the other case. In both instances, the tumors were suspended on distinct pedicles. The larger tumor which almost completely filled the bladder was attached to the bladder-wall by only a pedicle about two-thirds of an inch in diameter. The tumors were single; and the mucous membrane of the exposed surfaces was apparently normal.

Symptoms.—Our histories show that the characteristic symptoms of tumor of the bladder were present

in these cases. Definite hemorrhage was present in both cases. There was more difficulty and frequency and irritability in micturition than usual in most cases of other types. One patient had had trouble for four years, and the other for five years. The bleeding was painless, except for clots, though it was very severe in both cases. The blood was bright red. Between the attacks of bleeding, which usually lasted a few days, the urine cleared up completely, and several of the specimens examined microscopically showed no blood. One patient had experienced repeated very severe hemorrhages for over three years, and was quite anemic. Pain became severe, and evidently was caused by obstruction to the urethra. Catheters had been used in both instances, though only occasionally until just before coming for treatment. Frequency of micturition, from thirty minutes to one and one-half hours, had been gradually getting worse in one case; and the other patient was obliged to use a catheter continuously for several days before our last examination. This may have been due to the enlargement in the prostate, which was evidently a part of the trouble.

In summing up the clinical symptoms, we find that these patients had the same symptoms which accompany other tumors of the bladder, except in one point, and that was the bleeding. The bleeding in both of these patients came as a very sharp and severe hemorrhage. Each had bled down to the point of extreme weakness several times. I do not think we have seen in other cases of lesions of the bladder or kidney so much bright-red blood passed in such a short time.

Diagnosis.—The diagnosis, though at least suggestive, could not be established to a certainty in either of these cases until an exploratory incision was made. In the first case, of smaller tumor, which was associated with an enlargement in the prostate, there was so much bleeding as soon as the cystoscope was intro-

duced that the field was entirely obscured, and we were obliged to operate on this patient almost as an emergency to stop the bleeding.

In the second case, a male 50 years of age, the neoplasm was so large that it almost completely filled the bladder. Its surface was smooth, and appeared just the same as the mucous membrane lining the walls of the bladder which was congested and inflamed. The capacity of the bladder was from 6 to 8 oz., and there was no apparent abrasion in the mucous membrane at any point. A parallel case had not been seen before, but it was thought more than likely that the tumor, which was easily palpable above the pubic bone, was extravesical and pressing well down into the bladder. There was also much bleeding during this examination, and clots seemed to be squeezed out of the roof of the bladder. The bulging was more on the right side. Clear urine was common from both ureters.

Treatment.—The first patient was examined in our clinic August 2, 1907. We endeavored to check the hemorrhage, and get him into better condition before operating, but repeated washings of the bladder with different solutions seemed only to make the condition worse, while the patient was getting continually weaker and suffering greatly from clots. His hemoglobin was under 50 per cent. Under primary ether anesthesia we made a large suprapubic incision. The peritoneum was stripped back from the bladder, and the bladder opened, as usual. After cleaning out the blood-clots, the tumor, which was about an inch in diameter, was seen attached to the right anterior wall of the bladder. It was held high in the bladder by an intravesical enlargement of the prostate. It was first thought that the enlarged portion of the prostate and the tumor could be enucleated together. In starting the enucleation the tumor was freed from the bladder as soon as the

mucous membrane had been separated. It was then seen that the tumor was on a pedicle suspended from the wall of the bladder, and that it was not connected with the prostate at any point. The enlargement in the prostate was then removed. The bleeding stopped within a short time, and it was not necessary to pack. A double catheter was introduced into the urethra; the suprapubic incision into the bladder was closed loosely with catgut; and a rubber-tissue drain was placed down to the incision. After the operation there was very little bleeding, and the urine was clear in a few days. The rubber-tissue drain was removed on the third day. There was slight leakage from the wound only during the first week. The patient left the hospital on the 16th day, the wound being practically healed. He was discharged from our care at the end of three weeks. This man has been perfectly well since the operation. The last communication from him was dated November 24, 1913.

The second case was examined in January, 1912. Because of the uncertainty of the location of the tumor, a suprapubic incision was made for an exploration. On opening the peritoneum, it was readily seen that the large rounded tumor was within the bladder itself. The kidneys were palpated to rule out a possibility of hemorrhage from a renal tumor. They were found to be negative. The peritoneum was then closed and packed off, and the bladder was lifted up into the abdominal wound. A large incision in the roof of the bladder was necessary for the removal of the tumor. The surface of the tumor was smooth, and its appearance the same as the wall of the bladder. The pedicle was caught in a pair of clamps, and the tumor removed. There was very little tendency to hemorrhage, and a few catgut stitches in the wall of the bladder at the point of attachment of the pedicle controlled all oozing. As the bladder was dry, the wound in the dome was

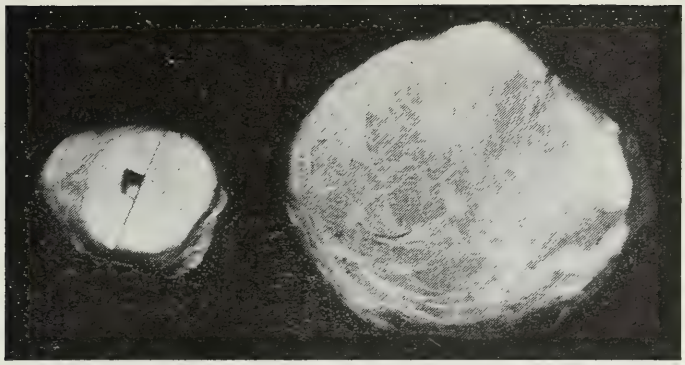

Fig. 1 Fig. 2

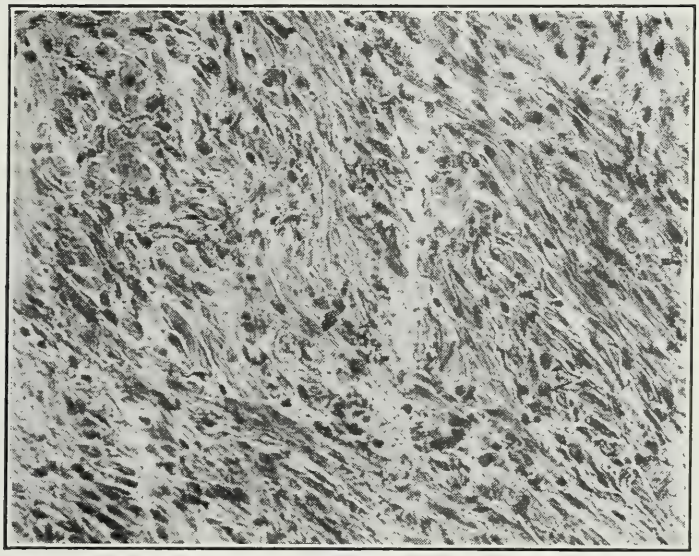

Fig. 3

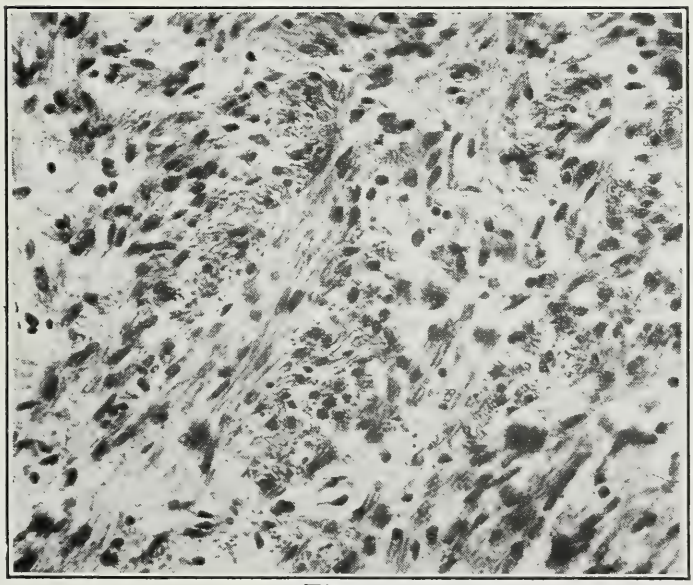

Fig. 4

closed completely, a rubber-tissue drain being left in the space of Retzius down to the opening in the bladder. Urine drained from the wound for some little time, and it was several weeks before it completely closed. After this rather slow convalescence the patient made a complete recovery. He had a little frequency and burning after going home. This man recently reports that as far as he knows he is entirely well.

Pathology.—The two tumors were very similar in structure. The smaller one (Fig. 1) weighed 40 grams, the larger (Fig. 2) 230 grams. They were rounded and very firm and hard. They were covered by stratified mucous membrane similar to the mucosa of the bladder. On cutting into the tumors they were found to have the same density throughout, and with very much the appearance of uterine myomas. Histologically they were composed of smooth muscle fibers and fibrous connective tissue. (Figs. 3 and 4.) The small tumor was much the more fibrous.

CASE REPORTS

CASE 1. (A446).—Male, aged 61 years. Date of first examination, August 2, 1907. Occupation, manager of an opera house. Previous diseases, pneumonia two years ago.

Subjective Symptoms.—Some bladder trouble for five years, starting with frequency and instability at times. Complains of difficulty in controlling bowels when passing the urine. Pain before urination. Lost ten pounds in past year. At times passes much fresh blood. At this time an examination of his urine showed pus and blood.

Hemogloblin—50 per cent.

After this examination, we did not see him again for four years. On November 28, 1911, we again examined him, at which time he said his symptoms had improved greatly after leaving here. Bleeding had ceased, and he had only occasional difficulty in urinating. During the past month, however, he had been much worse, with chills and fever and much blood in

the urine; had used a catheter entirely for the past five days, and was able to pass only a few drops without it. A week before returning he had had nausea and vomiting. He feels weak, and has lost 15 pounds in the last month. Examination of specimen at this time gives sp. gr., 1020; reaction, acid; albumen, +; erythrocytes, ++.

Objective Symptoms.—Bilateral enlargement of prostate, smooth and soft. Cystoscopic: prostatic enlargement, bilateral 3, intravesical 2 on a scale of 4. Bleeding raw surface of the left side of the bladder. Blood pressure, 150.

Diagnosis.—Prostatic enlargement.

Case 2. (A62,897).—Male, aged 50 years. Date of examination, January 9, 1912. German farmer. Family history, negative. Previous diseases, none.

Subjective Symptoms.—Comes for urinary difficulties with past history of repeated hemorrhages. Four years ago there was some moderate pain low in the right fossa. Three years ago there was painless hemorrhage of bright-red blood from the bladder; then no signs of any kind for two years. In the fall of 1910 there was second hemorrhage, consisting of dark clotted blood lasting five weeks. No pain. In the spring of 1911, there was hemorrhage lasting several days, and then off and on for several months. In the past four months much trouble and pain on account of obstruction both in passing of urine and obstruction to catheter. Painful experience four months ago on account of hemorrhage and obstruction, clots, and spasms. Occasional fever. Temperature, 102° at times. Much difficulty in urinating. Three weeks ago showed a growth in the bladder. Increasing frequency. Loss of weight, 10 pounds.

Objective Symptoms.—Pallor; large movable mass in suprapubic region a trifle toward the right side. Cystoscopic; mucosa shows chronic inflammation. No evidence of cancer or papilloma. A very large, smooth bulging mass as though the entire roof of the bladder were being pushed down. Much bleeding in clots seems to come from roof of the bladder.

Diagnosis.—Tumor in suprapubic region, either a part of the wall of the bladder or else extravesical pushing in the dome of the bladder. Urine analysis: sp. gr., 1012, acid, and considerable albumin.

In concluding, it seems that these two cases are of sufficient present interest to report because of their rarity. Of our entire series of 164 vesical tumors, they form 1.2 per cent. While they are rarely seen, it should be borne in mind that this condition is suggested in sharp, severe, painless bleeding from the bladder, not associated with other symptoms. It seems to us that it will always be difficult to make a positive diagnosis of the exact lesion in these cases without first exploring the bladder.

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DISCUSSION

Dr. William Jepson (Sioux City, Iowa): This is indeed a very interesting topic. I rise only to give expression to the limited experience represented by one case of this character, namely, a fibromyoma of the bladder, which emphasizes what Dr. Judd has said, namely, that difficulty must often be encountered in making a positive diagnosis. In this particular case the tumor was covered with concretions, which hid it and simulated a phosphatic vesical calculus, and under that impression the patient was operated on, and it was only after the bladder was opened that it was found to be a fibromyoma, with a small pedicle, which was readily removed.

Dr. A. C. Stokes (Omaha, Neb.): I wish to compliment Dr. Judd on his paper. I think his experience is unique. I have had an experience with one tumor somewhat different, in that the tumor was a myoma, which was buried in the muscle itself, with the exception that at one point there was some hemorrhage. A diagnosis was made from the hemorrhage and what we could see with the cystoscope, which was a large tumor on the right side of the bladder that appeared to extend into the bladder. Through a suprapubic incision, and with the aid of the cystoscope in the bladder, we slit up the mucous

membrane from the point where it bled, and dissected the tumor out from the muscle itself by the use of the fingers. On examining it we found it was nothing but muscular tissue.

These cases are very rare. I think every bladder tumor is potentially malignant, and it may be possible that we caught this previous to the time when malignant degeneration would have taken place. It seems to me that, as a general proposition, we should consider every bladder tumor malignant until it is proven to the contrary, and that it should be treated as such.

THE NECESSITY OF CONSERVING THE INTERCOSTAL NERVES IN ABDOMINAL INCISIONS: AN EXPERIMENTAL STUDY

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The longitudinal incision through the rectus muscle is of comparatively recent development in the various methods of opening the abdomen. It is used, not only for the purpose of making a more liberal opening into the abdomen in certain conditions, but also to permit the closure to be made in layers and by imbrication. The earlier longitudinal incision through the linea alba had several objectionable features. The most important of these were the high degree of traction necessary to overcome the bilateral pull of the transversalis and internal oblique muscles and the difficulty of obtaining solid union between the two edges of a single fascia. The incision was then made more and more lateral from the linea alba, until at present a great number of surgeons, both here and abroad, by preference make a liberal longitudinal incision through the rectus muscle at, or even external to, the central line of the muscle. This incision gives, especially in the upper abdomen, two firm fasciæ for closure, with the additional muscle cushion between them.

The incision is conservative with respect to the rectus muscle itself and perhaps to the deep epigastric and internal mammary vessels under the muscle; but

it cuts transversely through all other important structures of the abdominal wall. These structures are, chiefly, the fibers composing the fasciæ and the intercostal nerves. The severed fasciæ can be made to reunite, and permanent damage to them may not be great; but the destruction of the nerves causes an enervation of practically all the tissues which lie in the space between the incision and the linea alba. That this enervation frequently results in a weakening of the supporting tissues and subsequent herniæ has been known for a long time. But what about the other structures involved? What about the peritoneum?

From time to time I have had occasion to operate on patients who had previously been operated on through lateral abdominal incisions placed in such a way as to sever intercostal nerves. The findings in such cases have suggested a possibility that the omentum may have a greater tendency to adhere permanently to that part of the abdominal wall which has been deprived of intercostal nerve supply than to any other part of the parietal peritoneum. The omentum is ever ready to adhere to damaged tissues; and it would not be strange if it had this tendency also with respect to enervated peritoneum.

To illustrate some of the observations referred to: A woman of 40 was operated on for acute appendicitis. Misjudging the location of the appendix, the gridiron incision was made much too high. The tip of the appendix was found to be imbedded in the pelvis among loops of small intestines. The incision was then extended downward outside the semilunar line to a point near the groin. Naturally all nerve-fibers within the bisected zone were destroyed. There were no adhesions to the abdominal wall except at a point near the groin. Moist pads were used to wall off the infected region; and the appendectomy was done without unusual mauling of the parietal peritoneum. The upper

part of the wound was sutured, and a drainage tube was inserted at the lower end of the incision. The wound discharged a small amount of pus during the following week, but it was healed completely in three weeks. A year later this patient was operated on again for post-operative hernia. At this time the omentum was found to be firmly adherent to the whole area of parietal peritoneum, lying between the incision and the linea alba. Similar post-operative adhesion-formation has been seen in a few instances following a longitudinal incision over the gall-bladder. The omentum has been found attached internally from the line of incision, while on the external side the peritoneum appeared normal. It would seem that instrumentation- and possible bile-infection during the operation would have injured the peritoneum about equally on both sides of the incision.

A series of experiments on animals has been conducted by the writer, in an endeavor to learn whether or not the destruction of the intercostal nerves supplying the peritoneum would result in any anatomical changes in the peritoneum, with special reference to adhesion formation.

The plan adopted was as follows:

Group 1.—Under aseptic conditions, to make an incision near the costal margin on one side and sever the last three or four intercostal nerves (the 12th, or subcostal, is included under the term "intercostal" throughout this paper); after some time, to explore the abdomen and learn whether the destruction of the nerves alone caused any changes in the peritoneum supplied by these nerves.

Group 2.—To open the abdomen through the linea alba, in a number of animals, the incision to be placed in the lower part of the epigastrium, so that the peritoneum supplied by the last four intercostal nerves

could be easily reached; to traumatize the parietal peritoneum on each side of the incision and to exercise the greatest care that the degree of injury inflicted should be as nearly alike on the two sides as possible; to close this incision; and, lastly, to make a second lateral incision, and sever the lower intercostal nerves on one side.

Group 3.—To perform the same operations, traumatize the peritoneum, and cut off the nerve supply on one side, and to add an irritant or a mild infection to the peritoneum and, in the same dosage, to each side of the abdomen.

The trauma was inflicted by rubbing the peritoneum on both sides with fingers or forceps wrapped in a definite amount of gauze; by rubbing the same number of times and in the same direction on each side; and by using as nearly as possible the same amount of force to each side.

The irritant used was a weakened tincture of iodine. The infection was a solution of gastric or intestinal contents obtained from the same animal on which it was used. It was assumed that an autogenous infection of bacteria would be more easily overcome by the animal than foreign bacteria, which might originate an uncontrollable peritonitis.

All these means and methods are fairly accurate, and easy of application, except the necessity of applying an exactly equal amount and kind of force to each side in making the trauma. This must depend on the judgment and experience of the operator, and cannot be mathematically accurate. However, I believe it possible to exercise sufficient care in bruising the peritoneum by hand to make the margin of error almost negligible. In a series, this margin of error would probably break even for both sides.

Three experiments were made in Group 1, extirpation of intercostal nerves without disturbing the peritoneum. In Group 2, eight animals, five dogs, and three rabbits, were used. An incision was made in the linea alba, the peritoneum was rubbed equally on both sides, the wound closed, and then the lower intercostal nerves were excised on one side through a second incision. In Group 3, seven experiments were made. The peritoneum was rubbed on both sides through a median line incision, then tincture of iodine was applied in two animals, gastric contents in two, and intestinal contents in three. After closing the median-line incision, segments from the 10th, 11th, and 12th intercostal nerves were removed near the costal margin on one side. It might be added that the solutions used, both of iodine and of gastric and intestinal contents, were very weak.

Report of Operations and Experiments

GROUP I

Dog No. 1.—Operation, February 8, 1912. Under strict aseptic precautions an incision was made along the left costal margin from the epigastrium to a point near the crest of the ilium. The incision extended through the muscles and to the fascia transversalis. The last five intercostal nerves and the iliohypogastric were dissected out, and a segment of each extirpated. The wound was closed.

February 19, 1912, an exploration was made through a median-line incision in the epigastrium of the same dog. There were no adhesions to the parietal peritoneum anywhere. The peritoneum appeared normal. The wound was closed in layers, the peritoneum being touched only to suture the edges together. Another incision was then made along the right costal margin through the muscles and through the fascia transversalis. The last five intercostal nerves were dissected out, and a section removed from each. The wound was closed. All wounds healed without suppuration.

March 6, 1912, the abdomen was reopened for examination. The omentum was found slightly adherent over the middle

third of the peritoneal suture-line in the epigastrium. The peritoneum on both sides had a normal glossy and transparent appearance.

Dog No. 2.—Operation, April 29, 1912. An intestinal resection was made on this dog through a median-line incision at the umbilicus. The loop of bowel was lifted out of the abdomen, and surrounded with moist gauze during the operation. The parietal peritoneum was not touched with gauze. The wound was carefully closed in layers. A second incision was then made across the lower ribs on the right side, 3 cm. from the costal margin and segments from the 10th, 11th, and 12th intercostal nerves extirpated.

Autopsy, May 20. There were no adhesions to the peritoneum.

GROUP II

Dog No. 3.—Operation, March 11, 1912. A 10-cm. incision was made in the linea alba with the center of the incision at the umbilicus. The parietal peritoneum was rubbed with two fingers wrapped with four layers of gauze. The fingers rubbed rather gently forward and back twice on each side, care being taken to reach a point equidistant from the incision on each side, and as far as possible to apply an equal amount of force on each side. The wound was closed in layers. A second incision was then made along the right costal margin, and the five lower intercostal nerves severed and partly extirpated.

Abdomen reopened May 24, 1912 (74 days after operation). The omentum was found adherent along the greater part of the suture line, the adhesions extending 2 cm. to the left and 4 cm. to the right of the linea alba.

Dog No. 4.—Operation, May 30, 1912. Median-line incision 8 cm. long was made in the epigastrium. The parietal peritoneum was rubbed once forward and back on each side as far as the fingers could reach. The index and middle fingers were used, wrapped with four layers of gauze, fresh gauze being used on each side, and care being exercised to apply an equal amount of pressure on both sides. The wound was closed. A second incision was made along the left costal margin. The 9th to the 12th intercostal nerves, inclusive, and also the iliohypogastric were severed in the bottom of the incision; the vessels were ligated and the wound closed.

Autopsy, June 10, 1912 (11 days after operation). The omentum was adherent to the anterior parietal peritoneum over a triangular surface. The base of the triangle corresponded to the suture line under the linea alba and the apex to a point 5 cm. to the left of the incision. The triangle of adhesions lay entirely to the left of the incision. The adhesions were very firm; traction tearing the omentum did not loosen the adhesions. The peritoneum appeared thickened on the operated side.

Dog No. 5.—Operation, April 28, 1913. An 8 cm. incision was made through the linea alba, the umbilicus at the middle of the incision; parietal peritoneum was rubbed twice forward and back with two fingers wrapped in four layers of gauze; fresh gauze was used for each side and the same amount of pressure; wound closed carefully in layers. A second incision made at the right costal margin; the last four intercostal nerves were carefully dissected out through slits in the transversalis muscle, and the segments excised; neither intercostal vessels nor transversalis muscle fibers were severed.

Autopsy, May 13, 1913 (15 days after operation). There was a firm omental adhesion along the scar in the linea alba. The adhesion was practically confined to the right side of the incision and covered roughly a rectangular area 4x6 cm. in extent.

Dog No. 6.—Operation, April 28, 1913. Median-line incision 8 cm. long through the umbilicus; parietal peritoneum rubbed on each side with two fingers wrapped in three layers of gauze; three strokes (1½ excursion) were made with gentle and equal pressure on each side; wound closed with catgut in peritoneum and silk in fascia. A second incision was made over the left costal margin and the 9th to the 12th intercostal nerves, inclusive, were destroyed. Wound closed.

Autopsy, May 13, 1913 (15 days after operation). Omentum was feebly adherent over an area 5x5 cm. at the lower end of the incision. The adhesions extended an equal distance on each side of the incision.

Dog No. 7.—Operation, May 1, 1913. Median-line incision from 5 cm. above to 3 cm. below the umbilicus. Four layers of gauze were wound around two fingers which were introduced towards the lumbar region, pressed firmly against the abdominal wall and then withdrawn, causing a rubbing toward the incision. This was repeated three times on each side, each zone of rubbing being a little higher than the previous one.

Fresh gauze was used for each side. The abdominal wound was carefully closed with interrupted silk sutures in each fascial layer. A second incision was made across the ribs midway between the costal margin and the vertebral column on the right side. The last four intercostal nerves were dissected out between and below the ribs, and the segments removed. The intercostal vessels were not wounded. The wound was closed.

Autopsy, May 17, 1913 (16 days after operation). Omental adhesion along whole length of incision; adhesion 2 cm. wide and attached entirely to the right of the line of incision. Peritoneum thickened on the operated side.

Rabbit No. 1.—Operation, April 30, 1913. Incision through linea alba in epigastrium; peritoneum on both sides rubbed hard, once forward and back with one finger covered with gauze; wound closed; three lowest intercostal nerves on left side excised through incision across ribs behind costal margin.

Autopsy, May 12, 1913 (12 days after operation). Rabbit (female) found dead; had been pregnant and miscarried after the operation; right uterus found empty; left uterus contained unexpelled rabbits and placentæ, but the placentæ were detached. No visceral adhesions to abdominal wall; but on left side of linea alba was a transverse line of fibrin deposit adherent to the peritoneum.

Rabbit No. 2.—Operation, April 30, 1913. Midline incision in epigastrium; peritoneum rubbed firmly twice from the lumbar muscles toward the edge of the wound with artery forceps wrapped with gauze; the same on both sides; wound closed. Second incision at left costal cartilages and segments of the last three intercostal nerves extirpated.

Autopsy, May 19, 1913 (19 days after operation). Hernia of abdominal wall on left side; peritoneum roughened and omentum adherent at one point 2 cm. outside semilunar line on left side; adhesion 2 cm. in width.

Rabbit No. 3.—Operation, September 2, 1913. Incision through the linea alba from the center of the epigastrium to a point below umbilicus; parietal peritoneum rubbed equally on both sides with one finger wrapped in two layers of gauze, fresh gauze for each side; wound closed in layers with catgut. Second incision made across costal cartilages on the left side and sections of the last four intercostal nerves extirpated. Wound closed with catgut.

Autopsy, September 16, 1913 (14 days after operation). No adhesions to the parietal peritoneum; peritoneum appeared normal on the right side, while on the left it was thickened and had a dull grayish white color over the area between the incision and the costal margin.

GROUP III

Rabbit No. 4.—Operation, April 30, 1913. A midline incision, 5 cm. long, was made in the upper abdomen. One finger, covered with two layers of gauze, was swept over each side of the abdomen once forward and back. A piece of gauze carrying a few drops of tincture of iodine was touched gently to each side of the bruised peritoneum. Care was exercised that the sides of the abdominal wall should be given equal treatment. Wound closed. Second incision made at the left costal margin and the 10th, 11th, and 12th intercostal nerves and vessels severed. The incision severed practically all tissues down to the peritoneum.

The rabbit died three days after the operation. Autopsy showed the colon adherent to the anterior abdominal wall, the adhesions being decidedly more extensive on the left side. Intestine and liver stained with iodine. No adhesions between viscera.

Rabbit No. 5.—Operation, September 2, 1913. Incision through linea alba at the lower part of the epigastrium. Dry gauze was wrapped around a Carmault artery-forceps, and one hard rub made over the parietal peritoneum on each side; a piece of gauze containing a small amount of tincture of iodine was then touched to each bruised surface. The sides were treated as nearly alike as possible. After the treatment a faint iodine stain could be seen on each peritoneal surface. Wound closed with catgut. Second incision at left costal margin through most of the muscle fibers down to the peritoneum; the last five intercostal nerves were severed, and segments removed; three of the intercostal vessels were saved. Wound closed with catgut.

Autopsy, September 27, 1913 (25 days after operation). Whitened and thickened peritoneum over rubbed area on left side; right side normal. A lobe of the liver was firmly adherent at the central part of the traumatized area on the left side; no other adhesions.

Rabbit No. 6.—Operation, September 16, 1913. Midline incision at the center of the abdomen. Both sides of the

parietal peritoneum were rubbed once forward and back, and once in a dorsoventral direction with an artery-forceps wrapped in gauze. A small amount of gastric contents was obtained with a hypodermatic syringe and diluted one drop in 4 c.c. of sterile water. From this solution four drops were run over each side of the bruised peritoneum. Wound closed. Second incision at the right costal margin where the last four intercostal nerves were extirpated and nearly all tissue fibers severed to the peritoneum. Wound closed.

Autopsy, October 28, 1913 (42 days after operation). An adhesion from stomach to linea alba at central part of incision. Peritoneum is whitened on both sides of the middle line to nearly the same degree.

Rabbit No. 7.—Operation, September 16, 1913. Midline incision in epigastrium; with a forceps wrapped in gauze one rub was made over the peritoneum from before backward and another from the lumbar region toward the margin of the wound on each side. A hypodermatic needle was inserted into the stomach, and a few drops of liquid contents obtained. One drop of this was diluted with 4 c.c. sterile water and three drops from this solution were allowed to trickle over the rubbed peritoneum on each side. Abdominal wound was carefully closed. Segments were then removed from the 10th, 11th, and 12th intercostal nerves through a second incision at the left costal margin. Wound closed and sealed.

On October 28th (42 days after operation), this rabbit was again opened in the linea alba immediately below the previous operation. There were no adhesions to the abdominal wall anywhere. Condition of peritoneum not noted. Desiring to obtain a stronger infection, a hypodermatic needle was inserted into the colon. In the absence of liquid bowel-contents, it was found necessary to inject 5 or 6 c.c. of sterile water, which was again withdrawn through the needle. A 10 per cent dilution in sterile water was then made from the contents of the syringe. Four minims from this solution were then spread evenly over the peritoneum on each side of the incision. Wound closed.

Autopsy, November 20, 1913 (23 days after second operation). An omental adhesion was found to exist on the left side between the first incision (September 16) through the linea alba and the semilunar line. The adhesion was over 3 cm. wide. (This adhesion was not present at the time of the second operation, October 28th.) The peritoneum on the

left side was thickened and whitened from the linea alba to a line corresponding with the left costal margin. The peritoneum on the right side showed no such whitening, but appeared quite normal.

Rabbit No. 8.—Operation, October 28, 1913. An incision was made in the linea alba above the umbilicus, 10 cm. long. With a finger wrapped in four layers of gauze, the peritoneum was rubbed six times from before backward. A solution was then made with a hypodermatic syringe from colon contents in the same manner as in rabbit No. 7. Six drops of this solution were spread evenly over each side of the bruised peritoneum. Wound closed. The 10th, 11th and 12th intercostal nerves were then severed along the costal region on the left side. Another nerve, apparently an accessory from the 12th intercostal, was dissected out and excised. All intercostal blood-vessels involved were severed and ligated. Wound closed.

Autopsy, November 20, 1913 (23 days after operation). The peritoneum was whitened opposite the midline incision on both sides. The whitening was decidedly more marked on the left side than on the right. There were no adhesions to the line of incision, but on the left side in the center of the whitened area 5 cm. from the linea alba was a firm omental adhesion 2 cm. in diameter.

Dog No. 8.—Operation, November 5, 1913. Incision through linea alba at umbilicus 10 cm. long; the peritoneum rubbed on each side four times forward and back with forceps wrapped in gauze; liquid contents obtained from a loop of ileum with needle. This was diluted 1 to 10 in sterile water, and three drops of this applied to the rubbed peritoneum on each side. Wound closed. Second incision across lower ribs on right side and 10th, 11th, and 12th intercostal nerves severed 5 cm. behind costal margin; all intercostal blood-vessels intact, hence may not have destroyed all nerve filaments.

Autopsy, November 19, 1913 (14 days after operation). Peritoneum whitened and thickened on both sides opposite abdominal incision, but decidedly more so on the right where it extended beyond the costal margin; on the left it extended but slightly beyond the semilunar line. Omentum adherent to the central portion of the incision. The adhesion extended 4 cm. toward the right side, while on the left it barely crossed the linea alba.

In looking over the results from the experiments in Group 1, we find that the absence of intercostal nerve supply alone, without the addition of trauma, caused no adhesions and no macroscopic changes in the serosa. No sections were made in these cases to compare the two sides microscopically.

In Group 2, absence of intercostal nerve supply plus trauma, there is a difference between the two sides of the animals in all cases but one. The difference varies in degree, but it has a constant relation to the side deprived of nerve supply.

The individual resisting or recuperating power which varies so widely in the human, undoubtedly plays a role in lower animals also. This resistance depends upon tissues aside from the nerves, and may account in some measure for more thorough restitution in one enervated peritoneum than in another.

The rubbing in the first experiments was done with less severity than in the later cases. There was no precedent to follow, and it seemed best to feel the way gradually. The amount of force applied to the peritoneum was probably not much greater that that exerted by some surgeons while pushing gauze pads roughly into the abdomen, and, after the operation, carelessly jerking the pads out. In the later experiments the rubbing was done a little more energetically, yet in no instance to such a degree that subserous hemorrhages could be seen.

The peritoneum in the rabbit is very thin and delicate; and undoubtedly it received a much harder rubbing in proportion to its strength than did the peritoneum of the dogs. As a probable result of this there was found in the rabbits a more constantly appearing infiltration of the peritoneum than in the dogs. The omentum of the rabbit is very short, and does not lend itself to adhesions as readily as that of the dog. In most of the dogs there were omental adhesions along

the line of the peritoneal incision in the linea alba. This can be accounted for chiefly by the fact that the edges of the peritoneum were rubbed and handled more roughly than the distant parts. Tincture of iodine was used very freely on the skin; and some of this drug, no doubt, added insult to the peritoneal margin. But in all cases where adhesions extended farther from the linea alba on one side than on the other the adhesions favored the enervated side.

Microscopic sections taken from the whitened areas of the peritoneum on the enervated side, and compared with similarly situated sections on the normal side, showed that the subserosa was much thicker in the former than in the latter. This thickening seemed to be due to a round-cell infiltration and an edema, which was absent on the normal side, two to three weeks after the operation. This tends to show that repair is delayed where the nerve supply has been removed. It proves that nature makes a noble and partly successful effort to repair the damage even in the absence of nerve supply, but that this repair suffers from a lack of that control, which makes for rapid and perfect result.

A mild trauma or infection on a healthy peritoneum induces a temporary adhesion, which may be released after the healing process of the serosa has been completed and absorption has taken place.

These experiments suggest that in the absence of intercostal nerve relations the infiltration becomes more chronic and the healing defective; and therefore a more reluctant release of the adhesions is to be expected.

Very decided results were seen in the two rabbits where iodine was applied to the peritoneum. One died, undoubtedly from bowel-obstruction due to the adhesions to the parietal peritoneum. The adhesions were much more extensive on the enervated side,

though by no means confined to that side. In the other rabbit the whole rubbed area on the operated side was whitened and thickened, and a lobe of the liver was adherent in the center of the area. The peritoneum on the other side appeared normal.

The combined injury from the rubbing and the iodine was probably greater than that applied in any other experiment. This suggests that, if a higher degree of damage was inflicted on the peritoneum than that used in any of these experiments, the difference in reaction between the normal and the enervated side would be much more striking.

The two experiments with gastric contents were practically negative. The rubbing was made with forceps wrapped in gauze, and was applied more gently than in most experiments. The gastric contents of a rabbit probably contain but few bacteria; and in the small dose applied to the peritoneum even those few may have been missed, so that no additional reaction was called forth.

The intestinal infection used was very feeble, if indeed present in all cases. Yet in all the three instances where it was applied after rubbing, there were omental adhesions on the operated side, and none on the normal side. Of special interest is the case where no adhesions were present after the application of gastric contents, but an adhesion formed after colon-infection was added.

In the case of a female rabbit having a miscarriage after the operation, it is interesting that the uterus on the side of intercostal-nerve extirpation had been unable to empty itself. Whether this was merely a coincidence, or whether the nerve destruction on that side had any influence on the uterine contractions, it is impossible to state.

If only the trunks of the intercostal nerves are extirpated at the costal margin or even between the

ribs, one cannot be certain that the whole area supplied by these nerves is totally enervated. This may be true even if we overlook the probability that the nervi vasorum have some function beside that of strict vasomotor control. Nerve filaments may come off from the main trunk behind the point where the nerve is severed, and pass unharmed in the transversalis fascia in the zones between the nerves. To obviate this possibility, the lateral incision in such experiments should sever all the tissues down to the peritoneum. This is exceedingly difficult to do without damaging the peritoneum, and it was attempted in only a few of the experiments. It is probable, therefore, that the enervation was not so complete in any of the experiments as that produced in a laparotomy with a lateral longitudinal incision.

The question of time was not considered in these experiments. The adhesions as a rule covered only a small part of the rubbed area; and the more recent ones were not very firmly attached. It must be admitted that some of them might have disappeared in course of time; but the majority of the adhesions certainly were too firm to promise any such outcome.

An analysis of the results of the fifteen experiments in which trauma, or trauma plus irritation or infections, was applied on the parietal peritoneum shows that in eight the adhesions (seven omental and one hepatic) were confined to the side of nerve-extirpation; two of the three cases having adhesions on both sides of the linea alba had more extensive adhesions on the operated than on the normal side; one case had fibrin deposits, which were limited to the enervated side; and one rabbit having no adhesions had a marked infiltration of the peritoneum on the operated side, thus leaving only three animals in which no material difference between the two sides was noted. An ineffectual attempt to infect the peritoneum with gastric

TABULATION OF EXPERIMENTS AND RESULTS.

Experiment No.	Trauma &c. inflicted to peritoneum each side of linea alba	Inter-costal nerves cut	Examination (exploration or autopsy)	Results
GROUP I.				
Dog No. 1....	Abdomen not opened.....	Left....	11 days after op.	No adhesion, peritoneum normal.
Same dog.....	" " "	Right...	16 " " "	Slight adhesion to incision, peritoneum normal.
Dog No. 2....	Intestinal Resection..... (no rubbing)	Right...	21 " " "	No. adhesion.
GROUP II.				
Dog No. 3....	Rubbed with gauze.....	Right...	74 " " "	Omental adhesion extending 2 cm. to left and 4 cm. to right from linea alba.
Dog No. 4....	" " "	Left....	11 " " "	Omental adhesion 5 cm. wide, entirely to left of linea alba. Peritoneum thickened on left side only.
Dog No. 5....	" " "	Right...	15 " " "	Omental adhesion 4x6 cm. confined to right side.
Dog No. 6....	" " "	Left....	15 " " "	Omental adhesion 5x5 cm. distributed equally on both sides.
Dog No. 7....	" " "	Right...	16 " " "	Omental adhesion 2 cm. wide, all on right side. Peritoneum thickened on right side.
Rabbit No. 1..	" " "	Left....	12 " " "	No adhes. Fibrin deposit on left. Incomplete ahort in left and complete in right uterus.
Rabbit No. 2..	" " "	Left....	19 " " "	Omental adhes. 2 cm. to left of lin. semilun. Hernia on one side.
Rabbit No. 3..	" " "	Left....	14 " " "	No. adhes. Peritoneum whitened and thickened on left side.
GROUP III.				
Rabbit No. 4..	Gauze rubbing plus Tr. Iod..	Left....	3 " " "	Colon adherent both sides, mostly on left.
Rabbit No. 5..	" " " " " ..	Left....	25 " " "	Liver adherent on left side. Peritoneum thickened on left.
Rabbit No. 6..	Gauze rub plus gast. cont..	Right...	42 " " "	Stomach adher. to incision. Peritoneum the same on both sides.
Rabbit No. 7..	" " " " " ..	Left....	42 " " "	No adhesions.
Same Rabbit..	Added intestinal contents...	Left....	23 " " "	Omentum adherent and peritoneum thickened on left side.
Rabbit No. 8..	Rubbing plus " " ...	Left....	23 " " "	Omentum adherent and peritoneum thickened on left side.
Dog No. 8....	Rubbing plus " " ...	Right...	14 " " "	Omentum adherent and peritoneum thickened on right side.

contents had been made in two of these three negative experiments.

It can be stated, therefore, that in 80 per cent (12 in 15) of the experiments made to determine whether or not a destruction of intercostal nerves supplying parietal peritoneum is of pathological significance in regard to this peritoneum, the answer was, in greater or less degree, positive.

If further experimentation and observation should sustain the evidence which I have ventured to present, it will tend to discourage the use of every incision which deliberately separates peritoneum from its nerve supply. It will add another strong argument in favor of transverse abdominal incisions.

CONCLUSIONS

A destruction of the intercostal nerves without trauma to the peritoneum does not cause any macroscopic change in the peritoneum.

A perfect healing of a mildly traumatized peritoneum may occur after the intercostal nerve supply has been severed.

If a weak irritant or infection be added to the trauma, the serosa may still recuperate.

But if a peritoneal surface deprived of its intercostal nerve supply be subjected to a trauma, or to a trauma plus infection or irritation, such as may obtain during an operation, more adhesions and a more chronic infiltration of the peritoneum are likely to follow than if the same injuries were inflicted on a peritoneum with normal nerve supply.

I wish to express my appreciation to Dr. J. F. Corbett, Associate in Surgery at the University of Minnesota, for much valuable help and advice in this work.

Dr. Clifford W. Collins (Peoria, Ill.): My attention was first called to this subject by seeing an article by Dr. V. P. Blair, of St. Louis, on "The Conservation of the Parietal Motor Nerves in Abdominal Section," which was published in *Surgery, Gynecology & Obstetrics* for August, 1905. I became impressed with the advisability of protecting those nerves as far as possible, and since then my incisions have been planned with that end in view. At the meeting of this Association held at Minneapolis in 1908, I described a new incision for the right upper abdomen in which the skin, fat, and external wall of the rectal sheath were incised obliquely from the median line downward and outward. The rectus muscle was separated from its sheath, and retracted outward. The same oblique incision was then made through the posterior wall of the sheath and the peritoneum. In this incision there is no injury to the rectus muscle nor to the intercostal nerves that supply it.

After using this incision for several years I found one objection to it. It could not be extended upward or downward very easily after the oblique incision had been made. So I modified it by making the incision through the skin, fat, and external wall of the sheath of the rectus in a longitudinal direction over the rectus muscle and about an equal distance from each edge. The muscle is then retracted outwards, and the posterior wall of the sheath and peritoneum are incised longitudinally an equal distance from each edge. The muscle is easily separated from its sheath with the finger or some blunt instrument, except at the lineæ transversæ, where the muscle must be cut loose from the anterior wall of the sheath. This incision is non-continuous in that there is not a continuous opening down through the structures of the abdominal wall. When it is closed the uninjured muscle lies between the incisions in its sheath. The intercostal nerves that supply the muscle are not injured. It can be used over any portion of either rectus muscle, in the upper or lower abdomen, and can be extended in either direction.

It is very much like the Kammerer incision, except that in the Kammerer incision the rectus muscle was retracted toward the median line which was apt to damage the intercostal nerves.

While I am discussing incisions I would like to put in a word about the transverse Pfannenstiel incision for pelvic work. I have used it on 700 patients with such satisfaction

that it is a matter of some wonderment to me that it is not used by more surgeons. It is a non-continuous incision in that the only continuous opening through the abdominal walls down into the pelvis is one small point where the transverse incision across the aponeurosis crosses the longitudinal division of the recti muscles. This must make a strong closure. The upper transverse ledge of aponeurosis forms a natural protection to the intestines, and prevents the necessity of covering them with gauze pads with consequent abrasion of the peritoneal covering. There is no pull at right angles to the incision at any point and therefore no pull on the sutures. In six months the scar is usually invisible. That is the only objection to it. One of my patients visited another surgeon some time ago, and gave a history of a previous operation. When the surgeon examined her a pained look came over his face, and he said he regretted very much to inform her that he was afraid she had been imposed on, because there was no scar! Fortunately, for my reputation, the patient had accurate knowledge concerning the scar.

Dr. J. Frank Corbett (Minneapolis, Minn.): We have known for a long time that when two intercostal nerves were cut more or less anesthesia was manifest in the part they supplied. In the second place, we have known that cutting of the intercostal nerves had an effect on the muscles, and, as a result of nerve-destruction, we had muscle herniæ.

But this paper has called attention to another attribute of these nerves, and that is the effect that these nerves have on the repair of the peritoneum. I think the author has clearly shown that when the nerves are cut the peritoneum does not repair the same as when they are left intact.

A great deal has been said in regard to trauma as a secondary cause of adhesions. After reviewing this work, I would put the importance on the nerve supply above that of trauma.

Dr. Daniel N. Eisendrath (Chicago, Ill.): I think the point to which Dr. Quain has called our attention is one we do not pay enough attention to, namely, the preservation of the intercostal nerves. We are too reckless in our average incisions, and cut through nerves without considering the after-effects. I have found that for practically the majority of the work in the lower right quadrant of the abdomen the pararectal incision of Kammerer serves all purposes, especially if made at perhaps a little higher point than the ordinary incision. It will avoid the last intercostal nerves. It is an incision which is made with its upper border about the level

of the umbilicus and along the outer border of the right rectus muscle. Through that incision one can get at the appendix, the entire ascending colon, and, if you want to extend it upward, you can sacrifice one of the intercostal nerves, which comes below the umbilicus to expose the gall-bladder and pyloric region. One can palpate the right and left internal genitalia, and do everything one needs to in the right half of the abdomen. We found by accident through this incision during operation, and by systematic palpation, a good-sized left-sided ovarian cyst. The appendix was primarily diseased, but as long as we are inside the abdomen, no matter how straight the appendix history, I always put my hand down into the pelvis. I make it a routine practice, before I deal with a subacute or chronic case of appendicitis without suppuration, to introduce my fingers into the pelvis. If you make the incision in this way, with the upper border at the umbilicus, you can see the two intercostal nerves, one at the lower and the other at the upper edge of the incision, you can pick up the peritoneum, and the transversalis fascia can be seen as it curves around the middle of the incision. You can usually get through the lower portion of the transversalis fascia and peritoneum very easily, and have an ample incision to do everything, absolutely sparing the intercostal nerves.

Dr. Quain (closing): It is my opinion that transverse incisions should come into more general use than they are at present, in order to obviate the irreparable damage to the structures of the abdominal wall following the destruction of intercostal nerves. After convincing myself through these experiments as to the necessity of conservatism with respect to these nerves, I have practiced the transverse incision wherever possible, and the results have been very satisfactory.

There is another nerve supply to the peritoneum beside that from the intercostal nerves. Prof. Ramstrom (Upsala) demonstrated fine nerve filaments coming from the network of nerves accompanying the deep epigastric artery, running parallel with the artery for some distance, and finally ending in the peritoneum. What the function of these nerve fibers is, and what relation there may be between this function and that of the intercostal nerves are not known. Much more experimentation remains to be done before the innervation of the peritoneum is fully understood.

COINCIDENCE OF UMBILICAL HERNIA WITH GALL-STONES

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In the last few years in operating on a certain class of umbilical hernia I have been struck more often than not by the coincidence of the presence of gall-stones. Aside from the fact of their occurrence it has been interesting to consider whether or not there are etiological factors common to both, which make the occurrence more than a coincidence. The class of umbilical hernia referred to is that seen in adults all overly well nourished, and most of them from distinctly to enormously obese. For the most part these cases occur, or come for treatment, in the second half of life. My small series is composed exclusively of females. Denk[1], reporting the cases from April, 1901, to the end of December, 1909, at von Eiselsberg's clinic, shows the sex relation in a series of 89 cases to be 6 males and 83 females. This discussion takes no account of congenital umbilical hernia so-called, nor the herniæ in infants and children. All the cases in my series were parous. Of those reported by Denk, 79 out of the 83 were parous from one to fourteen times. In particular, then, this class consists of fleshy, middle-aged individuals, almost exclusively women who have borne children.

Of the 7 cases of this class of umbilical hernia to which I have called attention, 4 certainly had gall-

stones, which were removed at operation or, in one case, at autopsy, and of the others there is a probability in at least one more. The only references to the association of umbilical hernia and gall-stones that I have found in a somewhat cursory reference to the literature, is a report of the Mayo[2] clinic. In this instance the stones were removed at the time of the umbilical herniotomy. In the report of autopsy findings in the six cases of Denk's series, no mention is made of gall-stone disease.

For the purpose of this discussion the histories in detail of these cases are not essential. Besides gall-stones complicating the umbilical hernia in one case, there was a chronically inflamed appendix, which was removed; in another, an otherwise inoperable carcinoma of the uterus, which was curetted on account of hemorrhage. In one case the umbilical hernia was strangulated, and the skin showed signs of gangrene. Resection of some eighteen inches of small intestine and the greater part of the omentum was done. Infection superficial to the aponeurosis occurred, and spread extensively between it and fat, leading, in the course of six weeks, to a fatal termination. At autopsy, besides the gall-stones, an empyema of the gall-bladder and multiple abscesses of both kidneys were found. The resection and anastomosis, happily, had been perfectly successful. In respect to the gall-stone complication in these cases, the histories showed evidence pointing clearly to the condition; but, whether before as well as after the development of the hernia, my records, unfortunately, do not show. Aside from these features the cases corresponded to the type as already outlined. In other cases the symptoms of a pathological condition elsewhere, in the presence of a pregnancy at times, have been obtrusive; and the umbilical hernia in its incipiency, as it were, has been attended to as an incident to the other work.

If there is anything more than coincidence in the relation between umbilical hernia of the type indicated and gall-stone disease there must be predisposing, active, or exciting causal factors common to both which are operative. A summary of the alleged causes of these two conditions shows a striking similarity of etiology and symptoms in both diseases.

In the causation of umbilical hernia we have the principles of the causation of hernia in general, plus some peculiar features.

Fraenkel explains the development of adult hernia as a partial manifestation of congenital predisposition based on congenital clefts and pockets of elliptical form in the corpses of children intended for the passage of vessels and nerves which, on occasion, develop into hernia.

Eschenbach, on the contrary, thinks the congenital forms in adults are very rare, as, in but 1 case in 65, had the hernia existed since youth. Denk found but one case in 89. He sees in adipose degeneration and weaking of the aponeurosis the cause of hernia.

Championniere's theory assigns the ingrowth of preperitoneal fat into the interstices of the fascia leading later to the development of hernia. To this view Ruge adds the acquisition of fatty tissue as a cause.

Diakonow and Starkow explain umbilical hernia by the mechanical action of the rectus fibers being at right angles to the fibers of the aponeurosis.

Hiller's theory is based on the disproportion between the capacity of the abdominal cavity and its contents in view of the accumulation of fat in the mesentery and chronic tympany.

Denk's study of his material points to the Championniere-Ruge view of fatty degeneration of the parietal fascia, and states that 80 per cent of umbilical-hernia cases present marked or excessive adiposity of abdom-

inal parietes. Fraenkel's anatomical causation plays but a small part.

Coley's[3] explanation is, that "as the fatty layers of the abdominal wall increase the muscles decrease in size and tone, and the resulting relaxation affords a favorable condition for the development of umbilical hernia."

In this short view of opinions we see set forth the anatomical and pathological bases for hernia. Trauma as a cause is often assigned, but should be regarded merely as an exciting factor in etiology. Of 21 cases said to have been caused by trauma 18 were conspicuous for marked or excessive panniculus adiposus. My cases, with the exception of one (small hernia), operated on during second pregnancy incidentally to an acute appendix, were obese, and one enormously so. Obesity, adiposity, etc., figure prominently.

Of 6,220 (Berger) herniæ in the male there were only 134 umbilical herniæ, while of 2,229 among women there were 496. Men rarely have large umbilical herniæ unless having a general disposition to hernia or together with other herniæ. Of 134 cases in men over fifteen years old only fifteen cases occurred having an umbilical hernia only, while 95 had bilateral and 12 unilateral inguinal herniæ. In women of 494 umbilical herniæ, 438 cases of this form alone were present and only 56 combined with others. Of 496 umbiiical herniæ in women, 429 had had one and 377 of the number had had multiple pregnancies, and could with precision state that with each succeeding pregnancy the condition grew visibly worse.

Denk reports 4 nulliparæ as having umbilical hernia, but of them three were uncommonly fleshy, and the fourth, otherwise spare, had an enormous ovarian cyst.

With two exceptions the parous cases developed hernia only after delivery, showing that the straining and tension of this act have an unmistakable bearing as an etiological factor. Besides pregnancy more rarely large intra-abdominal tumors, high-grade ascites, and exudative peritonitis may distend the abdominal wall to the point of causing an umbilical hernia.

Here, again, the pathological fat seems to play its part. Of 21 cases of umbilical hernia after the first or second labor, 20 had marked or excessive obesity.

In respect to age after the first year in both sexes, umbilical hernia falls off rapidly in incidence, and in the male remains a rarity in later years. In the female between five and twenty-five years it also drops to a minimum; but after thirty years an increase in frequency occurs, and the type of hernia is entirely different.

Constipation associated often with chronic tympany, is common, and, not infrequently in temporarily incarcerated conditions, may increase to obstruction with the characteristic symptoms. Constipation may be a contributory cause or a result of umbilical hernia.

Having mentioned, briefly, the main features in the causation of umbilical hernia, it remains to consider those common to hernia and gall-stone disease.

In respect to age, we have seen that after thirty years the incidence of hernia began to rise rapidly. In my own cases all but one case were over forty and the other was over thirty. Nearly 50 per cent of all cases of gall-stone disease are people above forty[5].

In speaking of sex we saw the vast preponderance of this hernia in the female. In the incidence of gall-stone disease the proportion is about 3 to 1, corresponding with the incidence of the hernia.

In gall-stone disease almost as strikingly as in hernia, pregnancy plays a part. Naunyn has figured that

90 per cent of women with gall-stones have borne children.

Constipation, finally, is held to play a no inconsiderable part in the production of gall-stone disease as a starting-point by unloading onto the liver bacteria, more particularly of the colon group, in unusual numbers. The question of intestinal stasis as a causal factor in both this and hernia must be taken into account.

Obesity, although by no means essential to gall-stone disease, is certainly in women no rarity, but is, probably, rather the rule, yet by no means so conspicuous as in umbilical hernia.

Probably closely allied with constipation and obesity, and therefore with both umbilical hernia and gall-stone disease, sedentary habits and lack of exercise are matters of causal relation.

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CONSERVATIVE OPERATIVE TREATMENT OF LONG-STANDING INVERSION OF THE UTERUS

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From the time of Hippocrates, who wrote clearly of this condition, to the modern era, inversion of the uterus was treated principally by elevation of the lower part of the body and pressure on the protruding mass; that is, by posture and taxis. At first, posture seems to have been most emphasized, while, later, taxis, in its various forms, was more largely depended on.

In the long-standing cases, however, this treatment was only occasionally successful. Most of the women who survived the primary shock and the hemorrhage of the first few months dragged out a miserable existence, and finally succumbed to anemia or chronic sepsis. The repeated and prolonged taxis, so persistently employed, often aggravated the symptoms and hastened the end.

As early as 50 B. C. Themison suggested amputation of the bleeding and sloughing corpus uteri, stating that he did not believe that the uterus was essential to life. One hundred and fifty years later the suggestion was put into practice by Soranus, who amputated a gangrenous uterus; and the patient survived. From that time on, amputation of the bleeding corpus uteri was the usual treatment in those cases which re-

sisted non-operative measures. In more recent years complete hysterectomy (by vagina) was substituted, in the comparatively clean cases, for the simple amputation of the corpus uteri. At present hysterectomy is, I think, the treatment most commonly employed in the long-standing cases. In some of these patients, particularly in those past the age of 35, amputation or hysterectomy is the best treatment. In other patients, however, especially in younger women, sacrifice of the uterus is not the best treatment. In such patients, preservation of a functionating uterus is very important, and may be attained through a conservative operation. It is this phase of the subject that I wish to emphasize.

Conservative operative measures, that is, measures for the reduction and preservation of the uterus in the cases that resisted pressure replacement, have been developed only within the last half century. Aran, in 1858, and Marion Sims, a few years later, suggested incision of the fibers of the constricting cervical ring; but it is not certain that either of them actually used the method. The first recorded use of this treatment is by Robert Barnes, in 1861. In a case of inversion of six months' duration, he made three incisions in the constricting cervical tissues. The incisions extended deeply enough into the tense cervical ring to relax it, when taxis was applied, and the inversion easily reduced. In this method, as used by Barnes and others, the peritoneal cavity was, of course, not opened. On account of the danger of sepsis in those days, much care was exercised to avoid cutting through into the peritoneal cavity, hence the employment of multiple incisions to avoid too deep cutting at any one point.

The next radical step was the attempt to stretch the constricting ring by means of a dilator introduced through a median abdominal incision. In 1869,

Thomas reported a case so treated. He experienced great difficulty in executing the work in the friable infiltrated tissues of the long-standing inversion, and in a second case the attempt proved fatal. The method was exceedingly dangerous and only fairly successful, as indicated by failure in nearly half of the cases. In employing this method, in 1899, Everke aided reduction by superficial incisions into the constricting ring from the peritoneal surface.

In 1883, Browne employed an equally effective and much safer method of dilating the constricting ring. He made an incision through the posterior wall of the inverted fundus uteri into the inverted peritoneal pouch. Through this opening, about one and one-half inches long, a dilator was passed up into the constricting ring, which was then dilated. After careful and full dilatation, the uterine incision was sutured, and then the inverted fundus uteri was replaced by taxis.

Küstner (1893) made a long incision in the posterior uterine wall from near the fundus to near the external os. He first opened the posterior cul-de-sac by a wide transverse incision, broke up any adhesions present, and then incised the posterior uterine wall as just mentioned.

Following a suggestion by Piccoli, in 1894, Morisani, in 1896, extended the incision up the posterior wall through the external os, thus giving complete division of the posterior uterine wall, which facilitated reduction, in the old cases with infiltrated, sclerotic walls.

Spinelli, a few years later, incised the anterior vaginal vault, opened the vesico-uterine peritoneal pouch, and then divided the anterior uterine wall from the external os to the top of the inverted fundus. This anterior division gives as much facility in reduction in difficult cases as the posterior division; and, being anterior, it possesses certain distinct advantages.

Haultain, in 1901, attacked by the abdominal route an inversion of nine months' duration; but, instead of depending on dilatation of the ring, as did Thomas, Haultain incised the constricting ring posteriorly. The incision was gradually extended in the corpus uteri, as more and more of the inverted portion was pushed up within reach, until finally, reposition was effected. The replaced uterus presented a posterior incision about one and one-half inches in length, which was securely sutured from the peritoneal surface. The patient made a rapid recovery.

A few years later, Dobbin employed anterior incision of the constricting ring. It was a recent inversion complicated by sepsis. Taxis failing, he opened the abdomen, and endeavored to dilate the constricting ring after the method of Thomas. This failed completely. Then an incision was made in the anterior margin of the inversion funnel. This passed through the entire thickness of the uterine wall, and was extended sufficiently to permit replacement of the corpus uteri. The patient died some days later of the progressive sepsis.

In brief, the conservative operations for inversion of the uterus have been as follows:

OPERATIONS FOR REDUCTION OF AN INVERTED UTERUS

1. Multiple incisions into the constricting cervical ring. (Aran, Sims, Barnes, 1861.)

2. Dilatation of constriction ring by dilator introduced through an abdominal incision. (Thomas, 1869; with incisions, Everke, 1899.)

3. Dilatation of constriction ring by dilator introduced through incision in fundus uteri. (Browne, 1883.)

4. Division of constriction ring and adjacent uterine wall and cervix, posteriorly. (Küstner, 1893.)

5. Complete division of posterior uterine wall and cervix. (Piccoli, Morisani, 1896.)

6. Complete division of anterior uterine wall and cervix. (Spinelli, 1900.)

7. Division of constriction ring posteriorly through an abdominal incision. (Haultain, 1901.)

8. Division of the constriction ring anteriorly through an abdominal incision. (Dobbin, 1905.)

Vaginal operation is by far the safer plan. Complete division of the anterior wall of the cervix and corpus uteri (Spinelli method) is altogether the most satisfactory method, and the one I would strongly recommend in long-standing cases. The details of the method are conveniently shown by describing a case. The case reported is of interest also because of the seriousness of this affection and its extreme rarity.

REPORT OF OPERATION BY METHOD OF SPINELLI

Mrs. B., aged 23, operated on Feb. 7, 1913, for complete inversion of the uterus, of a year's duration, had been delivered of her first child Feb. 2, 1912. The labor was normal except for adherent placenta, which was removed manually. There was free bloody discharge and much cramping pain, which became less after six weeks. Bloody discharge continued off and on during the summer. The patient, though weak, nursed the baby, and was up and about most of the time. In the fall the bleeding became worse; and, finally, in the winter, the patient consulted Dr. H. S. Brookes, who examined her and made a diagnosis of inversion of the uterus. When I saw the patient, in consultation with Dr. Brookes, she was weak from chronic loss of blood, and presented the exsanguinated, waxy appearance so characteristic of profound anemia.

After preparatory treatment, to improve the patient's condition as far as possible, the inversion was operated on by the Spinelli method, as follows:

1. After disinfection of the vagina and inverted corpus uteri, the anterior vaginal fornix was incised and the bladder separated from the uterus: (Figs. 1, 2, and 3). The vesico-uterine peritoneal pouch was then opened; and the opening was widened laterally by the fingers, thus exposing the inver-

sion funnel, as shown in Fig. 4. Here there were numerous adhesions binding together the peritoneal surfaces; but the adhesions were easily separated.

2. The cervix was then divided anteriorly up to the constricting ring (Figs. 4 and 5); and the incision was continued down the anterior wall of the corpus uteri (Figs. 5 and 6). At various stages of the incision the attempt was made to turn the uterus inside in; but this could not be done until the incision was extended to the fundus.

3. The mucous surface of the uterus was then turned in by hooking the forefingers into the incision, and rolling the peritoneal surface out while the two thumbs, making counter-pressure, indented the mucous surface and pushed it in, as indicated in Figs. 7 and 8. Considerable stretching of the shrunken and infiltrated peritoneum was necessary at this stage of the work.

4. The thickened uterine wall was then trimmed away sufficiently to permit approximation of the peritoneal portion of the uterine incision. The turned-out endometrium and adjacent muscular wall had become greatly infiltrated and thickened; and, consequently, a considerable portion of this wall, amounting to about one-fourth of the entire uterus, had to be cut away as indicated in Fig. 9. This reduced the bulk of the uterus so that the shrunken peritoneum could be approximated around it.

5. The incision in the uterus was then closed by deep and superficial sutures. (Figs. 10, 11, 12, and 13.) The deep sutures included most of the thickness of the muscular wall, but missed the endometrium and the peritoneum. Over these deep sutures, and burying them, was placed a superficial suture, which approximated the peritoneum, care being taken to fold in the peritoneal edges.

6. Drainage was instituted. Free drainage is important in these cases because the chronically infiltrated and infected uterine wall cannot be wholly disinfected, and hence the incisions and manipulations spread contamination about the field. Marked absorption and fever for some days after operation is the rule in these cases; and in some cases the temperature is alarmingly high and persistent.

In this case tube-drainage was employed posteriorly, through a cul-de-sac opening (Fig. 14), and a rubber-tissue drain anteriorly. In a similar case I would now use a tube-drain anteriorly, as well as posteriorly (Fig. 14), to afford imme-

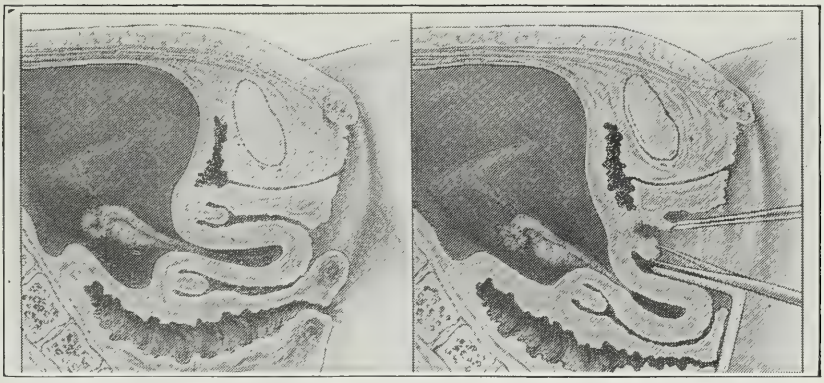

Fig. 1.—The inverted uterus, showing the changed relations.

Fig. 2.—Beginning the operation for reposition (Spinelli method).

Fig. 3.—Separating the bladder from the cervix.

Fig. 4.—The peritoneum opened, exposing the inverson - funnel. Division of the cervix begun.

Fig. 5.—Cervix and constriction-ring divided, and the incision being extended down the inverted corpus uteri.

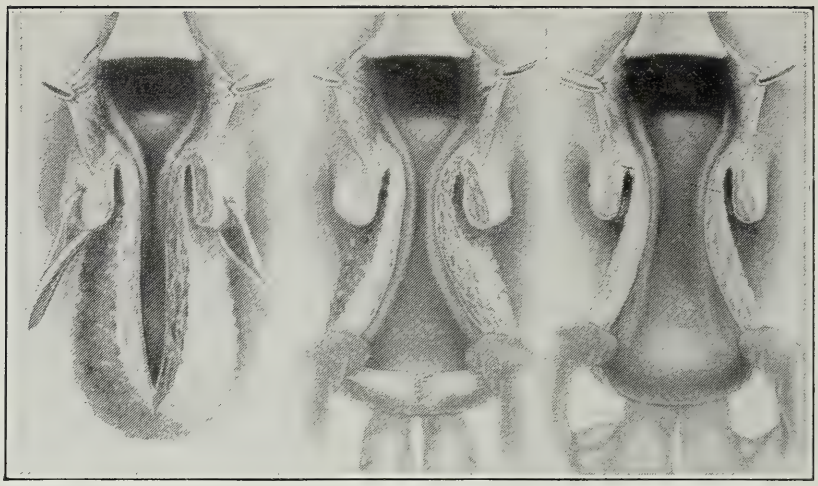

Fig. 6.—The incision extended to the fundus, as found necessary for reposition.

Fig. 7.—Turning the uterus inside in again. First step.

Fig. 8. — The turning almost completed. Peritoneal surface becoming external and mucous surface becoming internal.

Fig. 9.—Turning completed. Dotted line indicates portion it was necessary to excise on each side, to permit approximation of peritoneal edges.

Fig. 10.—Closing the uterine incision. One of the deep sutures for the muscular portion.

Fig. 11.—Closing the uterine incision. The superficial suture for the peritoneal portion.

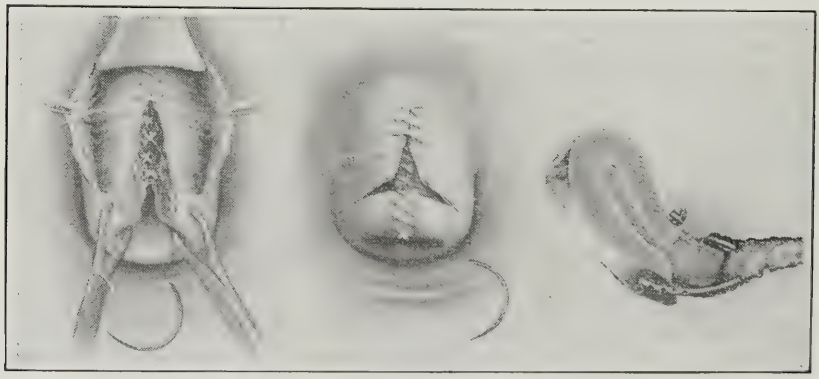

Fig. 12.—Closing the cervical incision.

Fig. 13.—Closing the vaginal incision.

Fig. 14. — Free drainage. Tube-drain in posterior peritoneal pouch and in the anterior pouch.

diate exit to any contaminated fluid that otherwise might accumulate in the vesico-uterine peritoneal pouch.

7. The vaginal-section opening was closed, with drainage, and a vaginal packing put in to keep the cervix well back and the fundus forward. It is important to take steps to keep the uterus forward during the healing process. If the tendency toward backward displacement is marked in the replaced uterus, it should be fastened forward by one of the vaginal operative methods. If there is no special tendency backward, a packing or pessary for some days will be sufficient.

Following operation the patient had considerable fever. The first day the temperature was 103° F., the second day 102.4°, the third day 102.6°.

This persistent fever was of course disquieting on account of the possibility of a spreading peritonitis or of generalized venous infection (pyemia). These dangers had been taken into account when I decided to save the uterus; and I had then determined that if serious infection developed after operation I would do a vaginal hysterectomy, to remove the infected organ and establish free drainage. It looked for a time as though a serious infection might be developing; but it did not develop. The fever was the only disturbing symptom. There were no evidences of serious peritoneal irritation, so I felt safe in waiting. The fever gradually subsided in the course of ten days, and the subsequent convalescence was uninterrupted.

The high temperature was probably an absorption-temperature, due largely, if not entirely, to the disturbance of the infiltrated uterine wall. There was not only an incision through the infiltrated wall, but also a general shifting of the relation of the elements of the wall by the forcible turning of the uterus inside in again. That this general shifting of the elements of the wall is an important factor is indicated by the fact that marked rise of temperature has been noted in cases of reduction by taxis without incision.

The patient gained strength rapidly, iron of course being given to overcome the marked anemia due to the previous chronic blood-loss. Menstruation began in April, the second month after the operation, and has been regular since. The first two or three times the flow lasted only two or three days and was scanty. Since then it has been normal,—from three to four days and a good flow, but not excessive. The patient states that the menstruation is the same now as it was before she became pregnant, except that the pains she formerly had do not now appear. The general health is good;

and the patient feels well and strong, and is as active as ever On examination recently, the uterus and other pelvic organs appeared practically normal.

ADVANTAGES OF THE METHOD

The advantages of the method employed are the following:

1. Being vaginal, it minimizes the amount of peritoneal contamination, a most important consideration when dealing with an infected structure.

2. As the reposition is accomplished by incision, there is not the bruising and perforation of the friable uterine wall which has so often accompanied attempted reposition by dilatation of the constriction-ring.

3. Division of the anterior uterine wall is preferable to division of the posterior wall, because the work is thus more easily and accurately accomplished. The anterior uterine wall and anterior fornix lie toward the operator, and hence are less deeply situated and more easily reached. Again, when the operation is anterior, the bladder may be lifted away, giving a wide space for investigation of the inversion-funnel and of the various pelvic structures, and also more room for the operative manipulations of incision, reposition, and suturing. If there is a marked backward tendency, effective forward fastening of the uterus may be carried out through the anterior incision. A suture-line on the posterior surface of the uterine wall extending to the fundus is more likely to form troublesome adhesions,—to the intestines, leading to obstruction, or to the posterior pelvic wall, leading to adherent retrodisplacement.

The points in favor of the posterior incision are that it eliminates the extra opening for drainage, and that the sacro-uterine ligaments may be more conveniently shortened; but these minor advantages of the posterior incision are outweighed by the more important advantages of the anterior incision.

Dr. Walter B. Dorsett, (St. Louis, Mo.): I would like to ask Dr. Crossen if he had any trouble in controlling hemorrhage after severing the adhesion or constriction?

Dr. Warren A. Dennis, (St. Paul, Minn.): An experience I had in the last year or so with a case of inversion of the uterus led me to look up the literature on the subject, and I must say that the illustrations of Dr. Crossen are the best I have seen. The most recent articles are not very well illustrated. The best is by Thorn, of Magdeburg. He has collected over 500 cases, including acute as well as chronic. From Thorn's report, or from his own statement, it would seem that he is entitled to have this operation called by his name. In the first place, Spinelli did it after Thorn, and employed a high vaginal fixation. Drainage was not recommended. There was no mortality from either the anterior or posterior operation. The anterior operation has been performed a few times, and always successfully. The posterior operation has been done a great many times without any mortality, but has not always succeeded in replacing the uterus. From the figures presented by Dr. Crossen, the anterior ought to succeed and is to be preferred to the posterior, although, in the past, in a good many instances the abdomen has been opened and the constriction cut, in order to reduce the displacement. This has been done by one of my professional colleagues satisfactorily, and in the present state of abdominal surgery it can be done safely. The mortality which came from this operation was largely in the pre-antiseptic era and was about twenty per cent.

Dr. Crossen's illustrations are most excellent, and the best altogether I have seen.

Dr. Crossen (closing): In regard to Dr. Dorsett's question, I would say that I had no trouble with hemorrhage. There are no vessels except the circular artery to cut, and we rarely have trouble in making division in the median line, which is the line of safety.

In regard to hysterectomy: That is usually indicated in chronic inversion in patients beyond thirty-five years of age. The uterus may give serious trouble afterwards, and if we save the uterus there must be something to offset that danger. When the patient is comparatively young we are justified in taking a good deal of risk to save the uterus. I feel that way, not in regard to this class of cases, but in other cases where

that question comes up. In a young woman under twenty-five years of age we can afford to take considerable risk of a secondary operation, for the uterus is worth that much to the patient.

Dr. D. C. Brockman (Ottumwa, Iowa): I can see how there may be trouble in the case of labor following this operation. Why would it not be better to do a hysterectomy? In a young woman we always try to save the uterus, but where we are liable to have dystocia it is a grave matter, and I think hysterectomy a safer procedure.

SUPRAPUBIC CESAREAN SECTION FOR PUERPERAL ECLAMPSIA

B. M. Ricketts, M. D.

CINCINNATI, OHIO

While deaths due to accident or disease during the pregnant state are perhaps the most deplorable, those due to eclampsia are more horrible and with a greater mortality, because two lives are concerned.

It is therefore no wonder that opinions concerning ways and means to overcome them should be at such variance. Not until a comparatively recent period in the history of dealing with eclampsia, have care-takers had to offer other than therapeutic and dietetic measures, surely not surgical measures, except it be phlebotomy, the value of which has probably been underestimated, delivery per vaginum by various methods not being considered in cases after the sixth month.

The great number of deaths occurring annually due to this fulmanant toxemia is sufficient within itself to excite suspicion that additional measures are necessary to overcome its ravages. It must be admitted, however, that those most interested in the subject are sorely at variance. But the squaring of the circle has been voted a possibility by a few who can locate the north star, describe the milky way, and estimate the courses and rate of speed of Halley's comet.

Enough evidence is offered in the accompanying reports and tables to change the existing angular thought to one more graceful and enduring. Though the hand

of the juggler may cause the compass to vascillate for the time being, its point will eventually direct the proper course to be followed in caring for the eclamptic.

The destruction of a city of large proportions by quake and fire was necessary to overwhelm and convince the world that there is something other than the roar of cannon and the glistening of swords.

It required the loss of the newest and largest ship ladened with the costliest cargo and the greatest number of human lives, to change the laws governing the navigation of the high seas. It was recently necessary for five million people to be overwhelmed by the sudden inundation of their homes, two hundred thousand of which were totally destroyed, to prove the fallacy of human endeavor; and it will ever be thus.

Zinke states (*Jour. of the A. M. A.*, July 26, 1913) that he has observed thirty cases of eclampsia resulting as follows: "Four mothers, 13.3 per cent, died; 15, or 50 per cent of the children were lost." The still high maternal mortality, 13.3 per cent, and fetal mortality, 50 per cent, in his last 30 cases, he says, was due to the fact that two of the mothers were moribund when first seen by him; one remained in profound coma after the first, and another after the eleventh, convulsion. The third died of shock and hemorrhage following an *accouchement forcé* performed by the physician in charge of the case. The fourth died soon after the eleventh convulsion, and a comparatively easy vaginal hysterectomy performed without an anesthetic. It is not claimed that the above mode of procedure will be invariably successful; but Zinke's experience impels him to believe that in those cases in which it fails, very little could have been expected from surgical intervention. Certainly, in the presence of any condition, maternal or fetal, which makes the birth of the child *per viam naturalem* hazardous or

impossible, abdominal or vaginal Cesarean section or deep cervical incisions, each depending on the period of gestation and other circumstances, is a justifiable operation. But in view of the evidence presented, it can only prove a serious error to maintain that an immediate interruption of gestation or termination of labor, by any surgical method in vogue, is the treatment *par excellence* in eclampsia. The good results obtained from strictly medical care in these cases far exceed the results accruing from all the surgical means proposed for relief from the disease.

Lutz (*Surg. Gyn., & Obst.,* p. 550, June, 1913) summarizes the following statistics of the Urban Lying-in Hospital from 1909 to 1912: 1 case of eclampsia in 107 deliveries; 24 per cent of the eclampsia cases occurred during the puerperium; the maternal death-rate was 6.7 per cent as a whole, 9 per cent during the puerperium, and 5.9 per cent before and during delivery. The fetal mortality was 32.7 per cent; but, excluding the post-partum cases, this rose to 36 per cent. The customary treatment of inducing labor was followed, rather than making a vaginal Cesarean section.

The labors terminated three times spontaneously, 17 by forceps, 13 by version and extraction, 3 by perforation, and 1 by vaginal Cesarean section. Venesection, morphine, and chloral were freely used. The author advises immediate delivery in severe cases where the pulse is small and rapid, the urine scanty, and coma persists between attacks.

On Aug. 4, 1913, *St. Ann's Maternity Hospital, St. Louis, Mo.* Dr. Percy H. Swahlen states that they have delivered from 96 to 117 per annum for five years with only three cases of eclampsia during that period, one coming on during labor, one four hours after labor, and one near the time of expected labor.

ECLAMPSIA STATISTICS

The great number of deaths occurring annually resulting from eclampsia in the United States should excite enthusiasm in devising ways to overcome its ravages. To show that this may be done the reports from various individuals and health officers of the larger cities have been assembled together with that of the United States Census Bureau.

There being no concerted action on the part of the states and territories to tabulate the deaths due to eclampsia, or any other cause, precludes the possibility of the United States Census Bureau making correct statistical reports. However, an attempt was made in 1908 to get the coöperation of the various health boards of the states to make them correct. It is interesting to know that there are many states that have absolutely refused to coöperate with the Census Bureau, and of the great difficulty it has had in securing the coöperation of those states within the census zone.

The Bureau cannot, therefore, give full detailed reports of diseases and their mortalities, though enough evidence has been secured from the more thickly populated states and cities to determine the approximate mortality of eclampsia, which is four to one hundred thousand population, or four thousand deaths annually in the United States. To the four thousand deaths of mothers may be added forty thousand deaths of children, making forty-four thousand deaths due to a disease that may be partially, if not completely, amenable to surgical, therapeutic, or hygienic measures, probably the three combined.

The Charity Service of Cook County, Illinois, reports: "We beg to state that during the past year we have had fifteen cases of puerperal eclampsia in this hospital, and of this number, seven have died."

Charity Hospital statistics of New Orleans, from June, 1908, to June, 1913, are as follows:

Parturition eclampsia, total, 33 cases: cured, 13; died, 20; 27 children cured; 6 died.

Pregnancy eclampsia, total, 7 cases: improved, 1; died, 6; 6 children cured; 1 died.

Puerperium eclampsia, total, 4 cases: 1 improved; 1, no record; 2, died; 3 children cured; 1, no record.

Total mothers, 44: 13, cured; 2, improved; 1, no record; 28, died; 36 children cured; 7 died; 1, no record. There was no Cesarian operations for eclampsia.

Munro Kerr Glascow Medical Hospital reports for fifteen years a mortality of 47 per cent in cases of eclampsia, without Cesarian operations.

Herman Trousac, "Medical Society of London," vol. xxv, p. 224, 1902, reports 38 cases of convulsions not operated on with 20 deaths. The following are the ages most frequently occurring:

Ages 10-19 263 cases.
Ages 20-29 981 cases.
Ages 30-39 700 cases.
Ages 40-49 146 cases.

HEALTH OFFICER'S REPORT

City—	1907	1908	1909	1910	1911	1912	1913	T't'l	Bl'k.
Cincinnati	1	8	5	6	8	8	....	36	....
Pittsburg	....	15	8	14	30	16	....	83	....
New Orleans	....	....	....	21	30	....	....	51	30
Chicago	47	55	50	56	60	50	....	318	....
New York City	....	127	160	138	143	161	....	729	....
Boston	....	12	39	20	27	32	....	130	....
Baltimore	28	24	30	22	17	20	20	161	....
Washington	....	17	8	8	15	20	....	68	....
Cleveland	....	6	5	18	12	13	....	54	....
Detroit	13	10	11	16	15	16	....	81	....
Providence	....	13	8	16	20	10	....	67	....
Nashville	....	6	3	3	8	5	....	25	13
Indianapolis	....	4	7	2	5	9	....	27	....
13 cities, total for five years	....	....	....	....	....	....	18	30	....
For the census zone only: U. S.	1,470	1,619	1,706	1,824	2,094	....	....	....	....

ETIOLOGY

The very interesting history of this disease is ancient and available in the various indexes are too well known now to receive other than passing mention. It probably occurs as early and as constant with animals as with man, though many appear to have escaped, indicating that it may possibly be a condition due to the influences of civilization and domestication. However, man appears to be the only one for whom it has a predilection.

The egg-bearing animals and birds are the only members of the two sub-kingdom vertebrata that are exempt from conditions simulating eclampsia and, so far as they are known, confined to the mammals.

CLIMATIC INFLUENCES

The reports at hand from health officers and hospitals of the larger cities indicate that there is a higher per cent of eclampsia and mortality in the Southern states, and that the per cent of cases and mortality is greater in the black than in the white race, there being no evidence at hand concerning the red and yellow races.

Statistics pertaining to eclampsia, like those upon any other subject, are more or less imperfect, and will remain so until legal measures are complete to make them perfect, but there is no excuse for lessening our efforts.

The high mortality among the people of the Southern states is probably due to unknown climatic influences; but with the absence of any evidence that the peoples inhabiting the arctic regions have a lower mortality than those of the middle and temperate zones, this question must remain indefinitely undetermined. With the absence of any information pertaining to the season of the year in which this disease is

most frequent, it might be reasonable to infer that its increased frequency and death-rate in the Southern states indicate that it occurs more frequently during the summer months. This would be a logical conclusion if it is found to be less frequent and deadly in the arctic regions.

Eclampsia seldom occurs before the fifth month of gestation, but gradually increases in frequency until full term of gestation, though it has been known to occur before the third month.

May not the exciting cause be found in the bite of an insect, the presence of a parasite entering the body by way of the mouth, anus, uterine cervical canal, or urethra or through the cutaneous structures?

If it is more frequent during the summer months or in a latitude where there are a greater number of warm months, would it not indicate the danger of insects or parasites, which are more numerous then?

What relation does the so-called eclamptic state in the female have to the so-called uremic conditions in the male?

The presence of albumin in the urine of the pregnant woman is not conclusive that eclampsia will occur with or without one or more convulsions, especially just before, during or after delivery, or before, at, or after full gestation, because convulsions do not always occur when albumin is present to any degree; indeed, eclampsia exists without convulsions or the presence of albumin. It may, however, occur after convulsions or delivery. It cannot then be considered a cause, but it may indicate the absence of an important substance; probably urea, that should be present in the urine in considerable proportion, but which remains in the general circulatory system, both arterial and lymphatic, as the result of acute nephritis varying in degree; though there does not appear to be any evidence indicating that even a large amount of urea is

essential to produce the required toxicity, small amounts being physiologic.

This would indicate that the amount of dosage varies with the individuals, and with the general condition of the individual at a time when the toxic irritation to the central nervous system is produced, if urea be a cause. But this remains undetermined. A certain unknown fulmanant toxemia is ascribed as being a cause.

Would the subjects of eclampsia having nephritis before conception have convulsions, so early in life, if they have never conceived? What role, if any, does an inhabited uterus play in the tragedy of convulsions? If the presence of a living or dead body within the uterus is the exciting cause, why do not convulsions always occur with such an inhabitant?

If the exciting cause exists within the mother and not within her uterus, why does not the living occupant of the body always have convulsions? Perhaps it does. It often has after birth. Its blood is identically the same in character as that of the mother, containing the same proportion of albumin and urea, which are supposed to have been the bane of the pregnant state. Is it the poison or the condition that produces the poison that causes the convulsion?

The presence of albumin may be due to neoplasm, urea, hysteria, epilepsy, cerebral irritation, diet, cardiac or hypatic disturbances. It is therefore important to differentiate them, though the same doubt prevails with their absence or presence.

If nephritis is due to the presence of gestation, and the two are the cause of eclampsia, should not gestation be terminated immediately? Should it not be terminated abruptly if the presence of the fetus alone has anything whatever to do with causing convulsions?

Surely, the presence of one dead within the uterus would not be tolerated longer than the time necessary

to remove it, in the shortest time and with the greatest safety to the mother. If necessary when dead it is more necessary when alive, because of the possibility of saving two lives.

The many theories pertaining to the reflex disturbances offer no solution to this all-important problem.

Gestation must be terminated by natural or artificial means soon after maturity (280 days), though the legal time has been extended to 320 days, with safety to the mother and the birth of a healthy, living child. This variation in time of gestation is mentioned to refute the statements that 280 days is the legal time, but these are not exceptions in favor of delay in delivery at any period of gestation when convulsions occur. The period of gestation varies with all animals and with the individuals of every particular kind.

In the absence of any knowledge of the unborn child having eclampsia simultaneous with the mother, would not investigation to prove or disprove their existence be important?

The child may be seized with convulsions before or after delivery, and die at once or survive for several hours as the result of toxins closely associated with uremia.

Antibodies have been suggested as the sole important cause of eclampsia; but, if they are, they have never been discovered.

CELIOHYSTERECTOMY (Porro)

1. Extraperitoneal stump
2. Intraperitoneal stump

Celiohysterectomy (Porro) implies the removal of the uterus with or without its cervical portion in the usual way, and, when applied to Cesarean section, removal of the uterus containing gestation at any period, with or without its cervical portion.

The necessity for removing a pregnant uterus arises when fibroids or infection are in evidence, and when the child has been dead for a number of hours. It can not be said that any form of hysterectomy should be done when the uterus is abnormal, or the pelvis is deformed for the purpose of preventing conception because the removal of the tubes alone will overcome that. To these may be added the ovaries in a limited number of cases when menstruation is to be prevented, but the ovaries should, when possible, remain undisturbed, because a normal is easier than an artificial menopause.

Extraperitoneal Stump, fixing the stump permanently in the abdominal wall, was formerly given preference. This method antedates the intraperitoneal stump method, and should continue to be the method of choice in infected cases and rupture of the uterus.

Infection is the most serious of the uncontrollable complications of pregnancy, that is to be reckoned with surgically, because of the high maternal mortality, with or without operation. For this reason a great number of operators refuse absolutely, and many hesitate, to render surgical assistance. Infection in the great majority of cases is primary to the operation; and the mortality is very high, while it is very low with infection secondary to the operation.

Detection of secondary infection, especially its type, before operation, is not always possible, but if it were the course of procedure should not be changed.

A dead child should, as a rule, demand this method of procedure, especially if it be dead for any great number of hours, because of the difficulty in determining the presence of infection or the state of decay of the child.

Uterine rupture, while not frequent, occurs often enough to be considered at all times; and when present

there should be no question as to the adoption of the extraperitoneal method.

The intraperitoneal method implies severing the body of the uterus from its cervical portion that the latter may be covered with peritoneum, and the abdominal wall closed immediately. This is the method of choice in cases of uterine neoplasms and anomalies, multiple pregnancies after the sixth month, and intra-abdominal neoplasms involving the uterus without infection.

INDICATIONS

1. Neoplasms of the uterus.
2. Intra-abdominal neoplasms.
3. Anomalies of the uterus.
4. Multiple pregnancy after the sixth month.

Uterine neoplasms vary in size, location, and number; and for this reason abdominal section should be done to determine the condition within that cavity, the course of pursual to be determined when the abdomen is open.

Intra-abdominal neoplasm, such as ovarian, mesenteric, retroperitoneal, and intestinal cysts and solid growths of any character are frequently associated with eclampsia to such a degree that the removal of the uterus is necessary by one of these two methods.

Anomalies of uterus are of many kinds, and often require hysterectomy by one of these two methods when eclampsia is present or absent.

Multiple pregnancy after the sixth month may be within the uterus alone, or within and without the uterus simultaneously, and when associated with eclampsia may probably necessitate hysterectomy by one of the two methods herein suggested.

SURGERY

1. Intraperitoneal
2. Extraperitoneal.

The purpose of assembling universal conclusions is to discover one composite which will direct a certain procedure, surgical or non-surgical, that will lessen, if not altogether eliminate, mortality.

The object of this work is, therefore, to supply data pertaining to eclampsia treated by suprapubic Cesarean section other than the lateral method.

The most exhaustive work of Dr. Ruben Peterson, of Ann Arbor, on "Vaginal Cesarean Section" is sufficient within itself to make it necessary to give that subject consideration.

Surgical.—The number of surgical aspects of puerperal eclampsia increases with knowledge of conditions and surgical technic. That the trend of relief is in the direction of immediate artificial evacuation of the uterus is self-evident, and that many cases may go to normal delivery with safety to both mother and child, there can be no question. But how to classify the two will probably remain in doubt. Until that doubt is eliminated ways and means must be considered that will offer the greatest safety to both the mother and child. It would therefore appear from the foregoing statements that the most rational method is offered in celiohysterectomy done by one of the various methods generally advocated.

Ambulancing.—Desperate symptoms demand desperate means of relief regardless of environment. One who is drowning cares not whether he is favored with a straw or a pack of cork so long as it is the best that can be had to save his life. A surgeon should not hesitate to demand that a Cesarean section be made

before manipulation of any kind is resorted to. Then, and not until then, will cases in the hands of certain practitioners have their best interests conserved. Time spent in dilating, or attempting to dilate, the cervix is wasted, and adds to dangers; such as infection, trauma, and hemorrhage incident to delay. It is to be lamented that the death of the child is given so little consideration, many operators having not made any report pertaining to its mortality.

Suprapubic Cesarean section is performed by two methods and their variations, namely, (1) intraperitoneal and extraperitoneal.

Intraperitoneal, the time-honored method, remains the one of choice, and should be given preference in the greater variety of conditions demanding the immediate evacuation of the pregnant uterus after the sixth month.

The transverse incision for abdominal Cesarean section was made in 1797 in Germany, but was little known, until Fenestiel made it popular.

Dr. J. L. Forwood has done forty-two intraperitoneal sections for all causes in ten years. He formerly lifted the uterus out of the abdominal cavity before incising it; but he now incises it, and delivers without doing so, the hands of an assistant being utilized for grasping the lower segment of the uterus to control hemorrhage. He does not cut low down in the lower segment.

EXTRAPERITONEAL CESAREAN SECTION

1. Lateral.
2. Median.

The lateral method has been done for many years, but has fallen into disrepute because of the very high mortality attending its doing.

The median method is of more recent origin, and bids fair to grow in popularity in properly selected cases.

Dr. Barton Hirsh, who is partial to this method and has done nine of the ten thus performed, describes it as follows:

"An incision large enough to permit the extraction of the child's head is made below the umbilicus. After making the incisions in the two layers of the peritoneum they are sutured together, which immediately closes the peritoneal cavity, making the operation extraperitoneal. Then follows the incision in the uterine wall, made in the ordinary way, and the extraction of the child's head with forceps. The lower uterrine segment is sutured with double catgut; and the abdominal wall is closed in the usual way. It has been found to increase hemorrhage if the placenta is delivered from the uterine wound. It is rather better to clip the cord off, drop it into the uterus, suture the uterus, and deliver as usual. If the woman is not in labor, it is necessary to extract the placenta through the uterine wound.

Dr. John B. Deaver inclines to this method, though he believes the danger to the child to be greater.

Dr. Wm. R. Nicholson, who has witnessed six operations performed by this method, advocates it in selected cases without infection, while Dr. E. E. Montgomery states that there is no great demand for the extraperitoneal operation in the absence of infection.

Dr. E. P. Davis welcomes most heartily a method through a peritoneal fistula, though he would not employ it in the presence of hemorrhage or infection or where infection is suspected. In our experience Cesarean section is indicated in eclampsia in not more than 20% of cases. It should, however, be promptly performed if improvement does not otherwise follow.

INDICATIONS FOR CELIOHYSTERECTOMY

After sixth month when associated with eclampsia.

In the order of their supposed frequency.

Any other condition or circumstance that would cause delay in prompt and rapid delivery.

An undilated cervix is a serious factor because of the want of uterine effort and the great danger through time necessary to dilate and deliver.

Deformed pelvis is prima facie evidence that the abdominal route should be selected.

A large fetal head is an indication that delivery through the abdomen should be resorted to, that the best interest of both child and mother may be conserved. This is done by shortening the time necessary for delivery, and avoiding a great amount of trauma incident to the route per vaginam.

A dead child (recently dead) should probably be removed suprapubically, oftener during the eclamptic stage than one that has been known to be dead for several days, because the dangers of infection are not so great with the first, while with the second it may be removed sectionally more rapidly per vaginam, and probably with no greater risk of infection.

Absence of labor pains, associated with eclampsia, demands careful consideration, especially at full term without dilatation of the os. Uterine contraction cannot be hoped for under such circumstances after delivery by any method.

Malposition of the child adds to the amount of time necessary to delivery, and must add to the danger to the life of both mother and child.

Neoplasms (intra-abdominal) are quite commonly present with the pregnant uterus at the time of eclampsia. Such complications are of many varieties, and, as a rule, demand celiohysterectomy that the greatest safety may be given to both the mother and child.

Elongated cervix, so frequently found in primiparæ, is a serious handicap when delivery is attempted per vaginam, especially when eclampsia is complicated with hemorrhages, even though the os be dilated.

Uterine hemorrhage, when uncontrollable, due to any cause, should be an indication for delivery suprapubically, especially if the loss of blood is sufficient to endanger life, because the further loss of blood to any considerable amount may be absolutely prevented.

Placenta previa, though rarely complicating eclampsia, is probably the most serious, because of hemorrhage, the danger of infection, and the jeopardy in which the life of the mother and child are placed.

Cervical neoplasms, malignant or benign, like scar-tissue, resulting from injury, disease, or surgical operation, especially amputation of the cervix, may be sufficient to prevent dilatation without the use of instruments.

Vaginal neoplasms of any character, or cysts, may, owing to their size, prevent or interfere with rapid delivery, and such pathology involving the rectum or coccyx, like their deformities, may also be troublesome.

Malformed vagina, such as may be congenital or acquired by injury or disease or surgical repair, is not infrequently encountered in delivery.

Deformed uterus may be congenital, or acquired by injury, disease or surgical operation, and when found to exist is a sufficient cause for resorting to the suprapubic route.

Rupture or perforation of the uterus absolutely demands abdominal section.

Labial hernia, though of rare occurrence, is a possibility, and when present is to be reckoned with as a serious condition, demanding the suprapubic operation.

Infection is the most serious regarding mortality, doubt always existing as to the propriety of doing any

kind of an operation whatever for the extraction of the child in the absence of eclampsia; but, when eclampsia exists, there should be no hesitation about emptying the uterus in the quickest possible manner, because the child, if dead, must not remain, though the mother may be reckoned fatally ill. If the child be alive and of seven, eight, or nine months gestation, it may survive though the mother does not recover. It would be criminal to permit the mother to die undelivered of a dead or a live child by some one of the recognized methods, one which would insure her the greatest safety so long as man is not infallible.

If the mother is moribund or convulsions are frequent or severe, the child should be saved by any means, rapid delivery offering the greatest safety. Duhrssn says operate after the first convulsion.

All surgical operations have passed through the period of condemnation, and celiohysterectomy for eclampsia is not an exception to this rule. The lowering of mortality each succeeding year with both mother and child is significant. The great length of time wasted, number of convulsions, degree of manipulation, trauma, unnecessary medication, and general deportment of the attendants, indicated by the reports herein contained prove that "sufficient unto the day is the evil thereof." Who would dare indulge in such practice with other emergencies? And yet the sentiment of the International Medical Congress, Geneva, 1896, concluded that "Cesarean Section, as well as forceps delivery, is not justified until all other methods have failed."

This sentiment prevails at the present time in private and public hospitals in the face of such evidence as is shown by the reports from the records herein contained.

The following tables and cases are submitted believing that they will add materially to the sum total in

solving this perplexing problem. Among these reports are many delayed cases with a high mortality, but those cases operated on early show a very low mortality, indicating conclusively the danger of delay to both mother and child.

Sloane Hospital for Women, New York, reports that there is no obstetrical service at Roosevelt hospital, and that Cesarean section for puerperal eclampsia has never been done at the Sloane Hospital for Women. *"The policy and teaching of this institution is against delivering such cases by abdominal operation."*

There are a score of reports of successful self-inflicted suprapubic Cesarean section, also reports of this operation having been done upon various kinds of animals with probably as favorable results as in man, but Cesarean section for eclampsia is of very recent origin, one of the first to suggest it being Lauverjat, 1810. It was, however, not performed until Akker, 1875 records the first suprapubic Cesarean section for that purpose. This was successful though he had the complication of a deformed pelvis.

Halbertsma, 1890, made three suprapubic sections for eclampsia, saving two mothers and three children; and Oldshausen, 1899, reports three such operations saving two mothers and three children. Kittlitz, 1897, reported 23 high operations with 11 deaths; and Hillmace, 1899, records 40 such operations with 20 deaths, thus showing a mortality of 50 per cent before 1900. Streickeisen, 1903, reports 26 operations with only 5 deaths.

Any discussion of Cesarean section *per se* would be incomplete without reference to the voluminous statistics of Dr. R. P. Harris (1885-90), Philadelphia. But as the following cases refer only to the suprapubic route for eclampsia alone his work will not receive further consideration. The same may be said concern-

ing the work of Dr. Ruben Peterson, who gives the following four tables to show the maternal mortality in prompt delivery, and the expectant treatment of eclampsia.

Prompt Delivery		Expectant Treatment	
No. of Cases	Mortality %	No. of Cases	Mortality %
615	98=15.9	390	113=28.7

Showing Results of Immediate Delivery and Conservative Treatment of Eclampsia

Immediate Delivery		Conservative Treatment	
No. of Cases	Mortality %	No. of Cases	Mortality %
150	6=4.0	147	46=31.2

Maternal Mortality After Spontaneous and Operative Delivery in Eclampsia Before 1900

Spontaneous			Operative		
No. of Cases	No. of Deaths	Mortality %	No. of Cases	No. of Deaths	Mortality %
1,126	263	23.35	1,443	406	28.13

Maternal Mortality After Spontaneous and Operative Delivery in Eclampsia Between 1900 and 1912

Spontaneous			Operative		
No. of Cases	No. of Deaths	Mortality %	No. of Cases	No. of Deaths	Mortality %
290	57	18.96	1,490	222	14.83

The fetal mortality by operative measures from 1900-12 has been reduced from 41-17 to 28.6 per cent.

In 315 by vaginal section the mortality was only 21.2 per cent when vaginal section was made; when only three convulsions occurred the mortality was only 11.8 per cent.

Ruben Peterson (*Surg., Gynecol. and Obst.*, p. 203, Aug, 1913) states that in 425 cases of eclampsia treated by abdominal Cesarean section, the maternal mortality before the aseptic era was 36.9 per cent and that in 317 of this number since 1900 the mortality has been reduced to 31.8 per cent. In 245 cases without infection the mortality was but 24 per cent. In 317 cases since 1900 the fetal mortality has been 5.5 per cent, and the mother but 3.7 per cent in 132 cases where the sections were performed after one to five eclamptic convulsions.

That the severity of successful and unsuccessful cases operated on has been greater than those treated medicinally there can be no question.

Peterson's final conclusion is, that the "operative procedure which will empty the uterus the quickest with minimum trauma and shock to the eclamptic mother and child should be selected."

Fetal mortality has generally been 44 to 54 per cent, but this high percentage has been reduced to about 25 per cent. Many children are saved though the mother be dying or dead, but few mothers are saved if the child be dead. This would of itself indicate that the majority of each may be saved with early operation—after the first convulsion or the beginning of the comatose state.

Eclampsia ceases more frequently after artificial than natural labor.

Dr. T. Halbertsma (Neberl. Weekbl. and Obst. Gaz., xiii, 1890, Cinti.) has performed Cesarean section in three cases of eclampsia. In the first case, operated on in 1878, the child was saved, but the mother died of peritonitis. In the second case, in 1888, both mother and child lived, the mother being discharged six weeks after the operation as perfectly well. In the third case the operation was performed after hypodermic injections of morphine and inhalations of chloroform had been tried without effect; mother and child were saved. The author recommends this mode of treatment on account of the very unfavorable prognosis of eclampsia.

Goltono (Gazz. med. di Torino, xlii, pp. 205-225, 1892) and Brothers (Amer. Jour. Obst., N. Y., xxxiv, 1896) each report upon this method of suprapubic section for eclampsia. Kittlitz, 1897, reported 23 cases of abdominal Cesarean section with maternal mortality of 50 per cent. Hillman, 1899, records a case stating

that the maternal mortality varies in 40 cases from 40 to 52.5 per cent.

Olshausen, 1900, performed abdominal Cesarean section three times out of his last 250 casès of eclampsia, saving two mothers and three children. Streickeisen, 1903, adds 26 cases to those of Hillman with a maternal mortality of 26 to 32 per cent. Sir. J. H. Croom (Trans. Edinburgh Obst. Soc., vol. xxxix p. 194, 1903-04) records two cases, the first operated on in Scotland; one a primipara, 20 years of age, near full term (a Porro), os undilated. Death followed six hours after operation; contracted pelvis. Second case, primipara, aged 46, contracted pelvis, elongated cervix, died at end of two days; child lived. Dr. F. J. McCann, 1908 (London Lancet, Sept. 10, 1910), reports a case of 26 years; primipara; os rigid and elongated; child born dead; mother lived; fits ceased after operation.

Dr. J. B. Murphy states that he has never seen a fatal case of Cesarean section, while Dr. E. C. Dudley says, "I regret to say that I have no experience, nor do I know of anyone who has had experience in the subject of abdominal Cesarean section for eclampsia."

Judd (Surg., Gyn. & Obst., p. 552, June 19, 1913) states that various observers believe Cesarean operation to be the safest in cases of placenta previa, and in some cases of eclampsia, and that uncontrollable hemorrhage is an indication for removing the uterus; although Cesarean section has not been done for eclampsia in the Mayo Clinic.

Boston Lying-In Hospital, July 3, 1913, replies: "We have had two cases of Cesarean section (abdominal) done for eclampsia without mortality. There have been five vaginal Cesarean sections for eclampsia. Of these, one mother died; three babies died, and one was still-born (five months miscarriage). Our visiting physician has performed two abdominal Cesarean sec-

tions for eclampsia in his private practice without mortality."

Massachusetts Homeopathic Hospital answers as follows: "During the year 1912, there were four Cesarean sections performed at this hospital for puerperal eclampsia, of which two died and two recovered. In 1911, there was one, who recovered. This year there have been thus far two such cases, both recovered."

E. P. Davis' reply of June 27, 1913, is as follows:

I have not kept separate statistics for puerperal eclampsia and Cesarean section, because I group eclamptic cases with infected and toxemic patients, and from the standpoint of Cesarean section as an operation, eclampsia is not especially interesting. My statistics of Cesarean section up to date are as follows:

Patients in fair condition, that is, not highly toxemic nor having streptococcic infection at the time of operation, 105. Maternal deaths, 1, from infection with bacillus proteus vulgaris. The source of the infection could not be ascertained by autopsy. Maternal mortality, .96 per cent. Fetal mortality in full term children in fair condition, *nil.*

Cases highly toxemic, including eclampsia, having pneumonia, heart disease, or septic infection, 30. Maternal deaths, 10. Patients recovering were those delivered by the Porro operation, and were septic at the time of operation. Fetal mortality in this series cannot be estimated, as all were in a highly diseased condition.

Post-mortem and moribund sections, 2. Total, 137.

The kinds of operation employed were as follows:

Celiohysterotomy, 81; celiohysterectomy, 34; Porro, with extraperitoneal treatment of the stump, 20; post-mortem and moribund sections, 2. Total maternal mortality of the entire series 7.9 percent. Mortality of cases in good condition, .96 percent.

Among these were 13 cases of placenta previa, treated by celiohysterotomy, with no maternal death.

As regards the treatment of eclampsia, I consider it a fulminant toxemia. We first bleed the patient, and give intravenous saline transfusion, the stomach is thoroughly irrigated, and five grains of calomel with soda placed in the

stomach. The bowels are thoroughly irrigated. Unless the patient is excessively relaxed she is put in a hot pack. If the os is dilated the membranes are ruptured. If labor develops, and can safely and speedily be terminated by forceps or version, this is done. If the patient does not improve, and if labor does not develop and the patient is at or near term, she is delivered by celiohysterotomy.

In our experience Cesarean section is indicated in eclampsia in not more than 20 percent of cases. It should, however, be promptly performed if improvement does not otherwise follow.

Dr. E. E. Montgomery states: "My experience is confined to one case that was no more than threatened eclampsia. Patient was seen on the 23d of February, 1912, and had almost complete suppression for twenty-four hours, then pregnant eight months. Under benzoate of calcium in plenty of water, the amount of urine was considerable increased; but the arterial tension, which measured 175 under nitroglycerine in .01 gr. doses was reduced only to 169. She complained of severe headache; pulse slow and tension very high. It was her first pregnancy, the superior strait was contracted and the vagina small. Under such circumstances I decided the chances for the child were best by Cesarean section. This was done on the 26th of February, and the patient never had an unpleasant symptom following. The child survived, and although it weighed but four pounds, it is now a strong, healthy child, and the woman has enjoyed excellent health ever since."

Dr. S. S. Halderman, Portsmouth, Ohio, reports as follows: "Mrs. K. white, primipara, comatose when first seen in June, 1909, but not convulsive. Celiohysterotomy. Recovery, uneventful. Mother and child living July 7, 1913."

Dr. A. H. Barkley, Lexington, Kentucky, July 20, 1913, reports the following cases:

Case 1. Mrs. J., aged 30, white, 1 child living. In labor five hours, and had two convulsions before first visit. At the end of the second convulsion the os was rigid, and the urine contained casts and albumin. No convulsions after operation. Fallopian tubes removed. Mother and child recovered.

Case 2. Mrs. R., aged 28, white. First child. Three convulsions before operation. Urine contained much albumin and many casts. Mother and child recovered.

Case 3. Anna M., negress. First child. Had six convulsions before operation, at which time the os was one inch in diameter, with contracted pelvis. Urine contained much albumin and many casts. Fallopian tubes removed. Mother and child recovered.

Case 4. Negress. Three convulsions before operation. Pelvis normal. Urine showed albumin and casts. No convulsions after operation. Mother and child recovered; the latter weighed 10½ pounds.

Case 5. Mrs. C. Third labor. Four convulsions before operation. Ovarian cysts in lower pelvis. Os 1½ inches in diameter. Bladder empty. Mother and child recovered. Chloroform anesthesia for the five cases.

Dr. Fitch, of Portsmouth, Ohio, July 8th, states as follows: "Mrs. H., aged 23, family history good. Eighth month of gestation. Saw patient first on April 22d, when she was recovering from convulsion, was cyanotic, cardiac action weak, unconscious. I made an examination, and found soft parts swollen, and a narrow contracted pelvis. Had council, and decided to remove her to hospital, and perform Cesarean section, without attempting to deliver by the vaginal route, as the patient was not in labor, and there was no dilatation of the os. Child was removed from abdomen in seven minutes, was weak, and weighed only three pounds, and died on the third day. Mother made a good recovery, and was discharged from hospital the 18th day, but had albuminuria for five weeks.

Dr. F. H. Williams, Portsmouth, Ohio, July 9, 1913, made a celiohysterotomy in 1905, for puerperal eclamp-

sia, without convulsions. Patient, aged 30, first child, comatose for thirty hours. Great adiposity. Child dead for several hours. Patient lived for twelve hours.

Dr. Stuart McGuire, Richmond, Va., July 15, 1913, states: "Have personally done two suprapubic Cesarean sections for puerperal eclampsia. Both women had gone to full term. Both mothers and one child lived. The os was dilated in one case, but labor had not begun in the other. Both cases were operated on within twenty-four hours after the first convulsion. One woman had never been pregnant before; the other had one child."

Dr. Thos. J. Watkins, Chicago, July 12, 1913, states that he has done one such operation for eclampsia at full term, with undilated os, four hours after first convulsion. First pregnancy; saved both mother and child.

Dr. F. F. Lawrence, Columbus, Ohio. July 19, 1913, states: "I have performed but one Cesarean section for puerperal eclampsia. That at the 7th month. The os was not dilated. The operation was performed 5½ hours after the first convulsion. It was the third pregnancy. Mother recovered. Child was not saved. There was but one slight convulsion after the operation. Because of the fact that there was a fibroid in the left anterior wall of the uterus, I performed a Porro operation. This was almost seven years ago, and the woman remains in excellent health."

Dr. Shelton Horsely, Richmond, Va., July 20, 1913, reports: "Two Cesarean sections for eclampsia. In both a typical abdominal operation was done. The period of gestation was between seven and eight months. In one case there were twins. In both instances the mother had been having convulsions for ten days, and was almost in a moribund condition and unconscious when they were delivered. Both mothers and all the children died. One mother lived two days,

and the other fifteen hours. All the children were born alive, but died within three days."

Dr. W. D. Haggard, 1907: "Cesarean section, multipara, nineteen years old. Pregnant eight months. No pains. Convulsions for several hours. Child and mother living four months after."

The Johns Hopkins Hospital states that they have treated 112 cases of eclampsia. Cesarean section was done in only two cases, and there were no fatalities.

City Hospital of St. Louis states: "In the year beginning April 1, 1909, and ending March 31, 1910, there were two cases, both received after delivery, and both died after Cesarean section, one child living and the other premature. In 1910-11 there were three cases, one of which lived twenty minutes after entrance; the other two also died, induced labor being performed; one child had been dead some days, the other child lived. In 1911-12 there was one case, not operated on, and lived. In 1912-13 we had two cases, not operated on, both living. Since April 1, 1913, we have had no cases of eclampsia."

Dr. John C. Altman, Nashville, Tenn., says: "I have had two cases of puerperal eclampsia, for which we did a suprapubic Cesarean section. Both patients were primiparæ; one white, and one colored. The colored one was sent into the hospital, having had a number of convulsions, and was markedly comatose. Upon examination I found a large fibroid in lower posterior wall of uterus. She recovered, child being dead before she reached hospital. The other case was 18 years old, primipara, 8½ months, 12 convulsions, cervical canal intact, no labor pains. Mother and child both lived. She has had two subsequent labors without complications. Time from first convulsion to operation, 10 hours.

B. M. Ricketts has done five suprapubic Cesarean sections, with but one for eclampsia, and Edwin Ricketts had five to record without any for eclampsia.

Case 1. Patient white, 33 years old, 150 pounds, married, well developed, primipara, excellent health during pregnancy. Examination of urine, negative until six weeks before delivery. No examination after that period. Supposed to be two or three weeks overdue. Slight headache on June 5th. Sudden and severe convulsions, 4 A.M., June 6th when Dr. Gillett was called.

Entered hospital 7 A.M., June 6th. Six convulsions from 4 to 7 A.M., and three from 7 to 8 A.M., making 9 convulsions before operation. Cervix undilated. Never any labor pains. Celiohysterotomy at 8 A.M. Chloroform narcosis. Child normal position. Child and placenta delivered within 90 seconds from time knife was applied. Total time consumed in operation, 33 minutes.

Severe convulsion at 8:50, 9:20, 10, 11 A.M., 12 M., and 12:45 P.M.; Total, 15 convulsions. None occurred after 12:45 P.M. From 9 A.M. until 3 P.M. one-half ounce of veratrum viride was given subcutaneously in 40 and 50 drop doses; with 130 grains of chloral hydrate and 240 grains of strontium bromide by the mouth. 12 pints of water containing ℥½ of bicarbonate soda and ℥i of chloride of sodium were given subcutaneously under the mamma. High delirium from 4 A.M., June 6th, to 9 A.M., June 7th. At 3 A.M., June 7th, the bed became saturated with urine passed involuntarily. Artificial heat and blankets from 8 A.M., to 11 P.M., during which time there was profuse perspiration, bowels moved freely on June 8th. Recovery uneventful, with primary union. Discharged mother and child June 24th. Both well and happy, though she did not supply a sufficient amount of milk for the child. There is no question of full term gestation.

In the case of Fitch the baby lived three days, though it weighed but as many pounds. That of Williams was hopeless from the late hour he first saw her, as were the two cases of Horsely, who was not called until the expiration of ten days. This feature of delay existed alike in the two cases admitted in the St. Louis Hospital and the two cases in the New Orleans Charity Hospital. The same can be said of the three deaths

of children and one mother in Johnson and Willis' ten cases.

Dr. Henry Schwarz, St. Louis: "Yours of the 8th inst. received in due time. If I understand you right, you are collecting data on cases in which women have been delivered by abdominal Cesarean section, and in which eclampsia furnished the indication for operation.

"I have delivered two women during eclamptic convulsions by laparotomy; the first at Washington University Hospital, October 25, 1911. Mrs. C., 20 years old, colored, at the end of her first pregnancy was brought to the hospital in convulsions; the uterus was small and empty; it was a full term ectopic, the sac being formed by the right tube; the child, a boy, lived 24 hours, the mother had had ten convulsions before, and an equal number after delivery, she remained unconscious for two days, but made an uneventful recovery.

"The second case took place at St. Ann's Maternity Hospital, December 9, 1912. Mrs. C., aged 17, at full term, was brought to the hospital because she had convulsions; examination revealed a closed cervix, an empty contracted pelvis, with a conjugate of 8 cm. and twin pregnancy. In this case I did a classical Cesarean section, the children, two boys, are living. The mother had three convulsions after delivery and remained unconscious for 24 hours; she made an uneventful recovery. So you see that in none of my cases did eclampsia furnish the indication for operation. I do not believe eclampsia as such ever forms a justifiable indication for Cesarean section."

Dr. John L. Forwood, Chester, Penn. (personal), September 17, 1903, states that he has done two Cesarean sections for puerperal eclampsia, each full term, os in each case dilated. One primipara with ten convulsions before operation and one multipara with twenty

convulsions before operation. Mothers and children all saved.

Nacke (Deutsche Gesellsch. f. Gynak, Halle, May, 1913) emphasises the importance of operative delivery, namely, the uterus is liberated from the dangerous muscular tension and the reflex irritation it induces. The pressure is removed from the abdominal vessels, especially those of the kidneys. The diaphram is allowed to recede, and the lungs and heart are not impaired. The delivery therefore accomplished the removal of a great number of complicating conditions, which alone may cause death, even without eclampsia.

Dr. C. S. Hutchinson, Los Angeles, California (personal), September 27, 1913, states that he has done one suprapubic Cesarean section for puerperal eclampsia saving both mother and child. Patient 22 years old, primipara, supposed eight and a half months gestation. On the evening before operation feet and hands badly swollen, considerable amount of albumin in urine. During the afternoon of the following day, she became convulsed, sent to the hospital, os undilated, and no labor pains. She had five convulsions within four hours at the end of which she was operated on.

PRE-OPERATIVE AND POST-OPERATIVE TREATMENT

Hot packs are more prompt and effectual than other heat-producing methods, though there be some danger in the wet garments remaining about the body unnecessarily. They are not so convenient as other methods, such as the hot-water bottles and earthen substances. The electric heater is infinitely better than all other methods, because it answers every purpose without dampness or the dangers of burn.

Nitroglycerine, $\frac{1}{50}$ to $\frac{1}{30}$ gr., given until its therapeutic effects are noticeable, has been recommended, but there is great doubt as to the advisability of resorting to its use.

Croton oil, given in two drop doses on sugar or flour, soon after the operation, is advisable in those cases attended with difficult evacuation of the bowels. These doses may be repeated if necessary every two hours until eight or ten drops are administered.

Calomel should be given in small doses combined with bicarbonate of soda immediately after the operation.

Ezerine would probably be an ideal remedy if injected into the ileum when the abdomen is open; otherwise it is not to be recommended.

Veratrum viride is the time-honored remedy, and when given in 25-drop doses every twenty minutes until the pulse is diminished in beats to fifty, is supposed to be by a few advocates a panacea. None but the best of the preparations should be relied upon. It would seem that the therapeutic effects of the drug have been overestimated, otherwise it would be more generally accepted and lauded. Then, too, toxic doses are ever looked for. It has an accumulative effect that results disastrously. The popular belief that failure to produce physiologic results is due to an imperfect quality of the drug, does not answer the argument against its use. Something more is apparently necessary to prove that veratrum viride will prevent or overcome convulsion in the eclamptic state. It may lower tension, but will not increase excretion of poisons. Will lowering pulse tension prevent convulsions?

Salt and soda solution.

1. Mouth.
2. Intravenous.
3. Subcutaneous.
4. Proctoclysis.

By mouth. If possible water should be given through the mouth and stomach by swallowing, or artificially through a tube, in large quantities.

Intravenous is the most direct, assuring more immediate effects, and when in proper hands is the method of choice. Any vein or artery may be appropriated though those of the arm are given first, and those of the leg second choice.

Subcutaneous method is more commonly practiced, being more generally understood and easily cared for, especially during the convulsive state. Great care should be exercised to avoid disarrangement or breaking of the tube or needle, an accident not uncommon during a convulsion. The point of insertion of the needle, which should carry a lumen $\frac{1}{16}$ inch in diameter, should be near the mammary glands as low as the umbilicus and extending to a line parallel with the nipples and laterally upon either side to the mid-perpendicular line, the amount varying from six to fourteen pounds, depending upon the body-weight, proportionate with the weight of the body.

Proctoclysis can best be resorted to when the patient is quiet or forcibly kept so, and should be continued until ten or fifteen pounds of normal salt solution have been absorbed at the rate of two drops per second, and of the body temperature.

Phlebotomy has been practiced throughout many centuries, supposedly with brilliant results. Indeed, with the obese and plethoric its benefits cannot be questioned, though other measures may be more appropriate. One to three quarts of blood have been extracted without serious results, depending upon the body-weight.

Bromides, soda, *potassium strontium,* etc., are no doubt of more or less benefit in eclampsia, varying in degree, but not at all curative or of benefit in convul-

sions severe in type. They are probably only adjuncts in their treatment. They may be given by the mouth or rectum in large doses.

Sodium benzoate, like the bromides, has been suggested, but the results have been equally unsatisfactory.

Chloral hydrate has for many years been a popular remedy, but doses large enough to be at all beneficial are more or less dangerous. Its use has deteriorated, giving place to more certain remedies. When administered, it should be given, well diluted, in large or small doses by mouth or rectum until physiologic effects are obtained.

H. M. C. is probably seldom indicated, but when convulsions continue, or there is a high degree of restlessness after operation, one or two doses given within two hours will prove beneficial, but like opium preparations is dangerous.

Pulmonary anesthesia by ether, chloroform, gas, or any of their combinations is dangerous to both mother and child, and should be condemned when other remedies can be obtained.

Spinal anesthesia is only mentioned to condemn it upon general principles, such as relate to cord injuries and a high mortality.

Pituitrine: Gorsew (Surg., Gyn. & Obst., p. 564, June, 1913) gives his experience in 48 labor cases, of which 25 are reported somewhat in detail, the author makes the following observations:

Pains begin in from two to ten minutes, accompanied by abundant micturition. Pituitary extract stimulates pains better during the second half of pregnancy, especially at its end; it gives good and reliable results in the first stage, and acts still better in the stage of expulsion. Anesthesia, and especially morphine, inhibits the action of the extract. It is more reliable than hot douches for effecting artificial premature labor. It frequently is able to supplant forceps, and

Kristellzr's expression. Pulmonary tuberculosis, diseases of the heart and kidneys, eclampsia, marginal placenta praevia, and premature detachment of the placenta he does not consider contra-indications. He relates that edema completely vanishes in from 8 to 19 hours, while the albumin markedly decreases or disappears from the urine after its use. It hastens the expulsion of the placenta. He observed no injurious effects on mothers or children.

In cases of atonic post-partum hemorrhage the extract gave reliable and permanent results, stimulating strong contractions of the uterus. He asserts that irregular pelves not below the medium degree of contraction are not contra-indications to the administration of pituitary extract.

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DISCUSSION

Dr. James F. Percy (Galesburg, Ill.): I would like to say a word or two on this subject because the essayist has not mentioned a very important prophylactic treatment of the condition that is back of eclampsia, namely, acute Bright's disease. Some of you will remember that I had the pleasure two years ago of bringing before this Association my experience extending over a period of more than four years with the use of the thyroid extract in all forms of albuminuria. Increasing experience with this method has convinced me if a diagnosis can be made fairly early, the woman will not have puerperal eclampsia, and therefore will not have to have a Cesarean section.

On my way to this meeting I was called to see a woman with a placenta previa, who also had a marked albuminuria. Her physician told me that he had had her on thyroid extract for two weeks, but, unfortunately, made the mistake that has unfortunately been made in the use of this agent, namely, that of giving only one grain every three hours. This is too small a dose and I said he should increase it to three grains every three hours. I also urged him to examine the urine daily, and if the albumin was not decreasing to give still larger doses.

There is no question but what Dr. Ricketts is right when it comes to the fully developed eclampsia, but I am talking now of the point in reference to the prevention of this condition. Thyroid extract, judiciously administered, will clear up many of these cases. I am getting reports from all over the country in reference to the use of this agent, and I hope within a year to report the experience that I have gained from these reports. In all cases where the patient is near term, I simply give the thyroid extract in what would be considered enormous doses,—ten grains, three or four times a day, or fifty grains a day if the urinary findings are alarming. As long as the patient shows any symptoms of hypothyroidism, the thyroid extract cannot do any harm, but can and will and always does an immense amount of good. The urine in three days, with large doses of the thyroid extract, will show a marked diminuition of albumin and casts, and in the larger number of the cases the results have been absolutely uniform.

I received a report recently from a physician in West Virginia who used the thyroid extract in the case of a colored woman. In her case the albumin was present in enormous amounts, as were casts. I may be enthusiastic, but the time is coming when this treatment will be, absolutely, universally used in threatened eclampsia when the profession knows of its great value.

Dr. Miles F. Porter (Fort Wayne, Ind.): It is well enough to call attention to the fact in connection with eclampsia that albuminuria itself is not by any means an indication of what the kidneys are doing. Many of these people with albumin in the urine have kidney sufficiency very much superior to others with no albumin at all. The kidney inadequacy may manifest itself upon the administration of iodide of potassium or of salt or of iodine, and not by the presence of albumin. Therefore, I would suggest that a diagnosis of albuminuria is not a diagnosis at all. It is like a diagnosis of ascites: it does not amount to anything, relatively speaking.

You want to find out the adequacy of these kidneys, not only with reference to the existence or absence of albumin, but what are they doing with the salt, or with the iodine, with the urea, etc. That is the way to test kidney-adequacy, and not depend upon any one thing.

There is another thing we ought not to forget, that many of these cases of eclampsia do not have convulsions until after the baby is born. The question is, what causes eclampsia? Eclampsia may result from different pathologic processes, which may or may not be improved by emptying the uterus. There is no routine method of treating eclampsia, any more than there is a routine method of treating so-called albuminuria. It depends upon what the eclampsia is due to.

Dr. Ricketts (closing): With reference to the remarks made by Dr. Porter, I would like to say it is true that puerperal eclampsia may occur after delivery, but the reports I have given you are those of men who stand high in the profession.

So far as the etiology is concerned, we know nothing. So far as albumin is concerned, we are in ignorance. I have not paid much attention to it so far as diagnosis is concerned. We do know, however, that the sooner a woman is operated upon, and the sooner the child is delivered, the safer it is for both mother and child, and statistics up to 1905 indicate the mortality of children to be 40 per cent. Here are two lives, both having an opportunity to be saved, and the life of the child is about as important as the life of the mother as it stands. We should not sacrifice the mother for the child when the child can be given an equal opportunity for its life.

I spoke about intraperitoneal and extraperitoneal incisions.

We have the so-called abdominal fistula advised by a gentleman in Philadelphia, who opens in the median line, brings the uterus up to the wall, and sutures the wall of the uterus to either side of the parietal peritoneum. He delivers through the two walls. In this way he thinks he prevents infection. Probably it is the operation of choice in those cases of infection or suspected infection. The other operation is that where the uterus is delivered and the child delivered outside of the peritoneal cavity, though it does not seem important to me whether the child is delivered before or after the uterus has been delivered.

I have been asked about hemorrhage. The hemorrhage in these cases is not profuse. All that is necessary is to have the assistant turn the uterus out before the delivery of the

child, put his hand on the broad ligaments, and press the vessels. He will then easily control the hemorrhage.

There are many other phases of this subject that I could present to you if time permitted, but I shall not do so. If there are any of you who would like any part of the literature on this subject I shall be glad to assist you in any way I can. I shall be glad to co-operate with you in sending reports of my own or those of others. When we consider that one-half of these operations are made by practitioners residing in the rural districts, and from whom we never hear, you will be surprised.

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CANADA

Winnipeg
MacLean, Neil John

INDEX

Abbott, A. W.
Discussion on cancer 323
Appendicitis, retrocecal 277
Appendix, the removal of the, in all cases of appendicitis with localized abscess 283
Bainbridge, W. S.
The significance of intra-abdominal "bands," "folds," and "veils" 161
Discussion on cancer 325
"Bands," "folds," and "veils," the significance of abdominal 161
Bartlett, W.
Discussion on bone cases 82
Discussion on ulcer 272
Basham, D. W.
Retrocecal appendicitis 277
Beck, E. G.
Tuberculous hip-joint disease 45
Discussion on hip-joint disease 66
Discussion on bone-surgery 119
Besley, F. A.
Pseudopancreatic cysts, with a report of four cases... 235
Discussion on pseudopancreatic cysts 241
Black, C. E.
Discussion on bone cases 80
Discussion on gas-bacillus infection 138
Bladder, non-papillary tumors of the 343
Blood-vessel injuries, the treatment of 29
Bone cases, report of interesting 67
Bowers, C. E.
Discussion on appendicitis 297
Brockman, D. C.
Discussion on pseudopancreatic cysts 241
Discussion on inversion of the uterus 388
Bursitis, subacromial 107

Cancer, inoperable, best methods of discouraging the activity of inoperable 311
Cesarean section, suprapubic, for puerperal eclampsia.... 131
Cholecystitis and the factors that control results of operations 207
Coffey, R. C.
Discussion on the duodenum 232
Cokenower, J. W.
Discussion on hip-joint disease..... 63
Collins, C. U.
Discussion on appendicitis 292
Discussion on intercostal nerves 370
Discussion on Jackson's veil..... 188
Colon, the best method of suspending the right..... 199
Common duct, repair of the, by means of transplanted fascia 217
Connell, F. G.
Discussion on Jackson's veil..... 181
Corbett, J. F.
Studies on the function of the periosteum..... 97
Discussion on bone-surgery 123
Discussion on blood-vessel injuries..... 35
Discussion on intercostal nerves 371
Crossen, H. S.
Conservative operative treatment of long-standing inversion of the uterus 379
Discussion of inversion of the uterus..... 387
Cysts, pseudopancreatic, with a report of four cases..... 235
Davis, B. B.
Discussion on cholecystitis 214
Discussion on Jackson's veil 185
Discussion on gas-bacillus infection 138
Davis, C. B.
Repair of the common duct by means of transplanted fascia 217
Discussion of the common duct 220
Dennis, W. S.
Discussion on the duodenum 231
Dorsett, W. B.
Discussion of inversion of the uterus..... 387
Discussion of Cesarean section 422

Duodenum, the; mobilization, traumatic rupture, and toxemia 221

Eastman, J. R.
Pericolic membranes 152
Discussion on appendicitis 291

Eclampsia, suprapubic Cesarean section for puerperal.... 389

Eisendrath, D. N.
The significance of the Jackson veil..... 142
Discussion on cholecystitis 212
Discussion on ulcer 270
Discussion on intercostal nerves 371

Exostoses, multiple: an hereditary affection of the bony skeleton 115

Fairchild, D. S.
Discussion on bone cases 83
Discussion on bone-surgery 120

Fay, O. J.
Parosteal callus: the so-called myositis ossificans traumatica 85
Discussion on bone-surgery 122

Freeman, L.
The use of wire tourniquet in operations for goiter.. 39
Discussion on avulsion of the scalp..... 132
Discussion on Jackson's veil 182
Discussion on bone cases 81
Discussion on appendicitis 293

Gall-stones, coincidence of, with hernia..... 373

Gas-bacillus infection 135

Goiter, the use of the tourniquet in operations for..... 39

Grant, W. W.
The treatment of blood-vessel injuries..... 29
Discussion on appendicitis 300
Discussion on the duodenum 233
Discussion on ulcer 270
Discussion on appendicitis 293

Gray, G. M.
Discussion on appendicitis 297
Discussion on avulsion of the scalp..... 132
Discussion on bone cases 80

Greenfelder, L. A.
Discussion on Jackson's veil..... 179

Haines, W. D.
Gas-bacillus infection 135
Discussion on ulcer 272
Discussion on sarcoma 340
Discussion on Jackson's veil 184

Hancock, J. C.
Coincidence of umbilical hernia with gall-stones..... 373

Harris, M. L.
Discussion on bone cases 83
Discussion on Jackson's veil 190

Hernia, coincidence of, with gall-stones..... 373

Hessert, Wm.
Report of interesting bone cases..... 67
Discussion on bone cases 84

Hertzler, A. E.
Discussion on bone cases 77

Hill, R.
Congenital pyloric stenosis 243
Discussion on ulcer 274

Intercostal nerves, the necessity for conserving the, in abdominal incisions: An experimental study..... 353

Jaskson's veil or pericolic membrane, symposium on.... 141

Jackson, J. N.
President's address 21
Discussion on Jackson's veil 194

Jepson, W.
Discussion on tumors of the bladder..... 351

Jonas, A. F.
Discussion on bone cases 78

Judd, E. S.
Non-papillary benign tumors of the bladder..... 343
Discussion on the common duct 220

Kanavel, A. B.
Duodenum, the: mobilization, traumatic rupture, and toxemia 221
Discussion on the duodenum 233

Kerr, A.
Discussion on Jackson's veil..... 187

Knott, Van B.
The removal of the appendix in all cases of appendicitis with localized abscess 283
Discussion on appendicitis 299
Discussion on Jackson's veil..... 184

Law, A. A.
Avulsion of the scalp 125
Discussion on the scalp 133
Discussion on bone-surgery 117
Discussion on Jackson's veil..... 186

Lewis, D. D.
Repair of the common duct by means of transplanted fascia 217

Littig, L. W.
Subacromial bursitis 107

Lockwood, C. D.
Ulcer of the stomach in children before puberty..... 255
Discussion on ulcer 275

Macrae, D.
Discussion on suspending the colon 203

Mann, A. T.
Discussion on the common duct 217

Mayo, C. H.
Cholecystitis and the factors that control results of operation 207
Discussion on cholecystitis 215
Discussion on the duodenum 232
Discussion on ulcer 270

Moore, J. E.
Studies on the function of the periosteum..... 97
Discussion on appendicitis 298

Nicholson, C. M.
Discussion on bone cases..... 82
Discussion on ulcer 274

Ovary, sarcoma of the 329

Parosteal callus; the so-called myositis ossificans traumatica 85

Pearse, H. E.
The best method of suspending the right colon..... 199
Discussion on suspending the colon 204

Percy, J. F.
Best methods of discouraging the activity of inoperable cancer 311
Discussion of cancer 327
Discussion of Cesarean section..... 421
Discussion on bone-surgery 121

Pericolic membranes 152

Periosteum, studies on the function of the..... 97

Peritoneum, surgical handling of the pus-infected..... 301

Porter, M. F.
Sarcoma of the ovary 329
Discussion on sarcoma 341
Discussion on Jackson's veil 183
Discussion on appendicitis 293
Discussion on Jackson's veil..... 183

President's Address 21

Pyloric stenosis, congenital 243

Quain, E. P.
The necessity for conserving the intercostal nerves in abdominal incisions: An experimental study.... 353
Discussion on intercostal nerves 372
Discussion on ulcer 273

Reynolds, W. T.
Discussion on hip-joint disease..... 66
Discussion on appendicitis 296
Discussion on Jackson's veil..... 187

Richter, H. M.
Discussion on ulcer..... 269

Ricketts, B. M.
Suprapubic Cesarean section for puerperal eclampsia. 389
Discussion of Cesarean section..... 422

Sarcoma of the ovary..... 329

Scalp, avulsion of the 125

Schnoor, E. W.
The significance of the Jackson veil..... 142

Stockes, A. C.
Discussion on tumors of the bladder..... 351

Tuberculous hip-joint disease 45

Ulcer in the stomach of children before puberty..... 255

Uterus, conservative operative treatment of longstanding inversion of the..... 379

Vaughan, R. T.
Multiple exostoses: an hereditary affection of the bony skeleton 115
Discussion on bone-surgery 123
Wallace, C. H.
Discussion on appendicitis 294
Witherspoon, T. C.
Surgical handling of the pus-infected peritoneum..... 301
Discussion on Jackson's veil..... 192

NB1004